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Inflation in the United Kingdom, 1948-1957[1]

By F. W. PAISH

PRODUCTION, REAL INCOME AND MONEY INCOME

Inflation may be defined as a condition in which the national money income is rising faster than the national real income. The most general cause of a rise in national real income is an increase in the output of goods and services; but over limited periods the national real income may also be considerably affected by changes in the investment income received from (or paid to) overseas countries and by changes in the terms of trade. During the years 1950 and 1951 the greater part of the rise in production was offset by a worsening of the terms of trade, so that real national income rose little, while since 1951 the recovery in the terms of trade has substantially augmented the effect on real income of rising production. Thus while production rose by an average of about 3½ per cent. a year between 1948 and 1951, real national income rose by only about 2 per cent. a year, while from 1951 to 1956, while production rose by about 3 per cent. a year, real national income rose by an average of about 3½ per cent.

TABLE 1

PRODUCTION, REAL INCOME, MONEY INCOME AND PRICES (1948=100)

Year	Production	Gross National Income		Ratio of Money Income to Real Income	Consumption Price Index
		Real	Money		
1951 ..	110·5	106·5	123·1	115·8	115
1955 ..	124·0	122·4	162·5	132·6	130
1956 ..	125·7	125·4	175·0	139·2	136
1957 .. (est.)	128	128·5	182	143	140

Meanwhile, money national income rose by an average of rather over 7 per cent. a year from 1948 to 1951 and by 7½ per cent. a year from 1951 to 1956; but since real income rose by 3½ per cent. a year in the later period and only 2 per cent. in the first, the rise in the ratio of money income to real income (which is a general price index for the total of goods and services produced in the economy) fell from 5 per cent. a year in the period 1948–51 to under 4 per cent. a year in 1951–56. In 1956–7 production has probably risen by about 2 per cent., real national income by about 2½ per cent. (thanks to the further improvement in the terms of trade), national money income by about 5½ per cent., and the ratio of money income to real income by about 3 per cent. The rise in the ratio of money to real income is slightly larger than the rise in the price index for consumers' expenditure, mainly because prices of goods and services bought by public authorities have risen rather more steeply than other prices.

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[1] *Memorandum presented to the Council on Prices, Productivity, and Incomes,* November 1957.

Cost and Demand Theories of Inflation

Two theories are current to account for the recent excessive rise of money incomes. One is that it is primarily due to the success of the trade unions in forcing up wages, and the other that it is due to a general excess of demand, which pulls up the price paid for labour in the same way that it pulls up other prices. My own view is that it is difficult to sustain the first theory in any circumstances without making assumptions which in effect amount to the existence of a demand inflation; but whether or no a cost inflation can exist in any conditions, it is even more difficult to advance it as the reason for the renewed rise in prices which has developed in this country since the temporary check in 1953.

If the original reason for the recent rise in prices had been the forcing up of wages by the trade unions against the opposition of the employers, one would have expected that in the initial stages of the renewed inflation the wage rises would have been, to some extent at least, at the expense of profits. In fact we find that, between 1953 and 1955, income from wages rose by 17 per cent. and gross trading profits of companies by over 24 per cent. There is no evidence here that wage increases were at the expense of profits. It is rather that business was so profitable that it was able to pay the increased wages and salaries while at the same time substantially increasing its profit margin. This would seem to support the view that wages went up, not so much because trade unions asked for higher wages as because employers could afford to pay them.

A second reason for doubting whether the actions of the trade unions in pressing for higher wages provide an adequate explanation of the rise in national money income is that wage rates, which are fixed by collective bargaining between employers' organizations and trade unions, have in most years risen considerably less than have the actual earnings of wage earners. Part of the difference can be accounted for by piece-rate earnings and part by more overtime;[1] but a large, perhaps the greater part, can be accounted for only on the assumption that many employers were in fact paying wages at rates increasingly above the trade union scale. This is again much more in conformity with a picture of wages pulled up by competition between employers for scarce labour than of wages pushed up by trade union pressure against the opposition of reluctant employers.

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[1] The average length of the working week as fixed by wage agreements fell by 0·2 per cent. between 1948 and 1951 and a further 0·2 per cent. between 1951 and 1956, while the average number of hours worked by all classes of wage-earners rose by 2·2 per cent. between 1948 and 1951, and by a further 0·6 per cent. between 1951 and 1956.

Only in three years since 1948 have earnings failed to rise more than wage rates: in 1952, in 1956 and probably in 1957. In 1952 and 1956 the percentage rise in earnings was almost the same as in wage rates, while in 1957 it was probably appreciably less. In all three years the Government, largely by the use of monetary policy, was trying to check the growth of demand, in all three there seems to have been a fall in profit margins, and in all three the average ratio of unemployment to vacancies was significantly higher than in years when earnings were rising faster than wage rates. While rises in the average level of wage rates remained large in the actual years 1952 and 1956, both were followed by years of much smaller wage increases. A rise of 8½ per cent. from 1951 to 1952 was followed by a rise of only 4½ per cent. from 1952 to 1953, while a rise of 8 per cent. from 1955 to 1956 has been followed by a rise of (probably) 5½ per cent. from 1956 to 1957. If these precedents are any guide, the rise from 1957 to 1958 should be appreciably less than 5½ per cent. The delay of a year before the effects of reduced demand are seen in a check to the rise in the yearly average of wage rates is explicable by the fact that most wage increases usually take place in the first quarter of the year. The greater rise in earnings than in wage rates in all years except those in which demand is to some degree checked seems to indicate that excess demand is at least a very important cause of the over-rapid rise of money incomes.

TABLE 2

WAGE RATES AND EARNINGS (1948=100)

	Average Index of Trade Union Wage Rates	Average Index of Earnings (men)	Earnings as a Percentage of Wage Rates
1951	113	119	105
1952	123	129	105
1953	129	138	107
1954	134	147	110
1955	144	160	111
1956	155	172	111
1957 (est.)	163	177	109

There is, of course, a sense in which it can be said that trade unions, by demanding higher wage rates, could generate the additional demand which would enable employers to pay them and at the same time maintain or increase their profit margins. Even though the Government, by limiting demand, could force employers either to refuse demands for wage increases or to cut down the number of their employees as the result of granting them, it might nevertheless regard the resulting strikes or temporary loss of output as politically unacceptable. The trade unions would then be in a position to oblige the Government to maintain conditions of excess demand in order to avoid strikes and maintain full employment. Strictly speaking, a politically secure government can always take steps to remove excess demand and check an inflation, but it is conceivable that trade unions could make the process so expensive in terms of strikes, unemployment and loss of output that all but the most courageous of governments might shrink from the attempt. In fact, however, it seems unlikely that the unions will press their demands for higher wages to the point either of prolonged strikes or of serious unemployment. History seems to indicate that the great majority of long and bitter strikes have occurred when the unions are defending either their real or money wages, or their own negotiating rights; and there is no reason now why money wages should fall or why wage earners should not receive their full share of the rise in real national income. If wage rates ceased to rise altogether, this rise in real income would appear in the form of a fall in the average level of prices, probably in the form of an appreciable fall in the prices of manufactured goods combined with stability in the price of services. Alternatively, average wages could rise proportionately with the national real income without bringing a rise in average prices. In this case a (smaller) fall in prices of manufactured goods would just offset a rise in prices of services. There is a good deal to be said for the second course, in the interests both of industrial peace and of the mobility of labour: it is easier to raise wages in one sector while keeping them stable in another than to raise them in one and lower them in another. In neither case should the rise in real wages be appreciably smaller than under a system of creeping inflation, where most of each rise in money wages is absorbed by the rise in prices.

CAUSES OF EXCESS DEMAND

Strictly speaking, the question of what causes an excess demand has no meaning; excess demand is produced by the demand for all uses jointly, and it is open to anyone to hold the view that, in some absolute sense, any particular use is too large. Neverthelesss, if we find that some particular use of resources has shown a marked increase proportionately to others during a period when excess demand has developed or been intensified, it does not seem wholly unreasonable to attribute the rise in excess demand to the increase in that particular use of resources. If this is so, it would seem that we can attribute the excess demand which has developed since 1953 to the rapid rise in investment, and especially in fixed investment, which has occurred since that year. We can distinguish five main uses of the national real income: (1) personal consumption; (2) public authorities' expenditure on goods and services; (3) fixed investment; (4) investment in increasing the volume of stocks; and (5) foreign investment, which, if we regard increases in the gold reserve as foreign investment, is identical with the favourable balance of payments on current account. Since 1953 these various uses of the national income are estimated to have been as follows:

TABLE 3

USES OF GROSS REAL NATIONAL INCOME (at 1948 prices)

	Personal Consumption		Public Authorities' Expenditure on Goods and Services		Gross Fixed Investment		Stocks		Balance of Payments		Errors and Omissions	Total
1953	9099	68·1	2306	17·3	1849	13·8	95	0·7	141	1·1	–133	13,357
1954	9503	68·4	2287	16·4	1995	14·3	35	0·3	175	1·2	–92	13,902
1955	9796	67·9	2255	15·6	2124	14·7	230	1·6	–59	–0·4	59	14,405
1956	9847	66·7	2285	15·5	2234	15·1	165	1·1	170	1·1	58	14,759
1957 (est.)	10100	66·7	2250	14·9	2300	15·2	260	1·7	170	1·1	50	15,130

If these figures can be regarded as giving even an approximate indication of the real position, it would seem that the use of resources on income account both by persons and by public authorities has absorbed a substantially declining share of real national income. The decline in the proportion absorbed by personal consumption is due to the very remarkable rise which has taken place in personal saving, while the fall in the share taken by public authorities is due to the success of the Government in stabilising its expenditure, in real terms, at the level reached in 1952. On the other hand, investment, both in fixed assets and in stocks, has absorbed a considerably larger share. Gross fixed investment, at 1948 prices, rose by over 20 per cent. between 1953 and 1956, while net investment, after deducting the amount estimated to be needed to make good wear and obsolescence, rose by nearly 40 per cent. Of the rise in gross investment, nearly 90 per cent. was in private industrial and commercial investment. It is this rise, superimposed on the earlier rise in (mainly private) house-building, which seems to have over-strained the resources of the economy and led to the development of excess demand. The causes of the uprush by nearly 50 per cent. in gross private industrial and commercial investment are no doubt complex. They probably include the introduction of the investment allowance, the effects of official urging, and perhaps a change in business sentiment following the earlier change of government; but almost certainly the lifting of restrictions on commercial building played a part, as the lifting of restrictions on private house-building had done earlier in stimulating the rise in the number of new houses built. It is one of the great disadvantages of physical controls that the amount of frustrated demand concealed behind them cannot be estimated, and that their removal may release a flood of demand which is unusually insensitive to other types of control.

In view of the heavy additional demand for resources occasioned by the great rise in investment, action designed to slow down this rise is not unreasonable; though it is, of course, open to anyone to maintain that the level of investment is still too low and that steps should have been taken to restrict consumption and promote still more saving rather than to control the rise in investment.

Fiscal and Monetary Policies

The distinction often made between fiscal and monetary policies for checking an excessive rise in money incomes is not a clear one; for fiscal measures may well have an effect on the quantity of money, as when an increased budget surplus is used to repay government debts to the banks. A more convenient comparison is between those measures which check the rise in incomes directly by withdrawing money from, or reducing its flow into, the income circulation, and those which seek to check it indirectly by withdrawing money from, or checking its flow into, the capital circulation, thus raising interest rates and discouraging investment. The most usual form which fiscal policy takes is to raise taxation without a corresponding rise in government expenditure, thus withdrawing money directly from the income circulation and either cancelling it by paying off government debt to the banks or transferring it to the capital circulation by paying off government debt to the banks' customers. The effect of this policy is to increase aggregate saving, reduce interest rates and stimulate investment. The return of the money to the income circulation, *via* increased investment, is, however, never complete, for the lower interest rates also reduce the (opportunity) cost of holding money idle and so increase the volume of idle money; further, what does return *via* increased investment returns to the income circulation only after a time-lag, the length of which depends largely on business expectations. Thus a continuing policy of using an increased budget surplus to pay off government debt will tend to bring a continuing check to the rise in the income circulation and in money incomes. Similar results are obtained by reducing government expenditure without a corresponding reduction in taxation, and by preventing money from being created, or transferred from the capital circulation, for the purpose of financing increased consumption (as, for instance, by restrictions on hire purchase finance).

The alternative policy of withdrawing money from the capital circulation, as, for instance, by paying off government debt to the banks with money borrowed from the banks' customers, reduces the supply of loanable funds in the market and causes interest rates to rise. A similar result follows from preventing new money from being created to replace the money withdrawn from the capital circulation to the income circulation to finance a rise in income. (This might roughly correspond to a fall in bank deposits, the money in which practically all capital transactions are carried out, equal to the rise in the issue of notes, the money used almost entirely in the income circulation.) The rise in interest rates has two effects: on the one hand it discourages investment, and so slows down the flow of money from the capital to the income circulation, and on the other it raises the opportunity cost of holding idle balances and so stimulates an increase in the velocity of circulation. The fall in the quantity of money in the capital circulation can, however, never be more than partially made good by releases from idle balances, since these releases take place only as the result of a rise in interest rates and are reversed if interest rates fall again.

If the rise in money incomes continues while the total quantity of money remains stable, more and more money will be withdrawn from the capital circulation, and since this loss can be offset only partly by the release of idle balances, rates of interest will continue to rise until the rise in aggregate money incomes is brought to an end. How long this will take, and how high interest rates will have to rise, depend largely on the expectations, in the early stages of inflation, of business men, but in the later stages also among consumers. If consumers are expecting prices to double each week, it may take a rate of interest of 100 per cent. a week to prevent them anticipating their next week's purchases; though even in this extremity the knowledge that the government was taking drastic action to stop the inflation might so affect expectations that a much lower rate of interest might be sufficient. In the early stages of an inflation, however, it is the expected profitability of investments rather than the expected rise in the price of consumption goods which is the incentive for the transfer of money into the income account; and at this stage a comparatively moderate rise in interest rates may be enough to reduce the profitability of investment sufficiently to end the net transfer of money to the income circulation. Generally speaking, it is the construction of the most durable assets which will be rendered relatively least attractive by the rise in interest rates, for the more durable the asset the higher is the proportion of interest cost and the lower the proportion of depreciation allowance in the total annual cost of ownership. High interest rates are therefore more likely to discourage the creation of long-lived, safe assets, such as houses and commercial buildings, where the rate of depreciation is very low, than that of machinery or vehicles, where it is relatively high. If, however, the rise in interest rates is expected to be temporary, investment even in durable assets will not be much discouraged if it can be financed temporarily on short term at rates not far above the long-term rate; for borrowers will hope to be able to obtain long-term re-finance at lower rates in the fairly near future. For high long-term rates to be effective they must be accompanied either by very much higher short-term rates or by quantitative restriction of short-term loans.

Both fiscal and non-fiscal methods of checking a rise in incomes face considerable difficulties. To be effective, increased taxes must be imposed on those who cannot maintain their expenditure on consumption either by reducing their saving or by spending capital: that is to say, on the general mass of taxpayers. And to increase taxation of this sort beyond a certain point may be both politically difficult and likely to have undesirable effects on the incentive to work. It was presumably considerations of this sort which prevented the then Government from increasing taxes in 1950–51 to keep pace with the rise in defence expenditure, and so preventing a sharp fall in the budget surplus, the rise in which had played so important a part in slowing down the rise in incomes in 1948–49.

The great difficulty in carrying out the alternative policy, of making money scarce in the capital circulation, is that the Government itself is now by far the greatest short-term debtor in the country, with not only some £3,000m. of Treasury bills in non-official hands but also many hundreds of millions of short-term bonds maturing every year. Any holder of these whom government policy makes short of money has only to decline to renew them on maturity to be sure of having his shortage relieved; and the only way in which the Government can meet these maturing obligations without creating new money is to borrow sufficient from the public (the banks' customers, not the banks) at whatever rate it is necessary to offer. Further, any other holder of maturing government short-term obligations, even if he does not need money himself, will refuse to subscribe to new short-term securities in their place unless he is offered as high a short-term rate as he can get elsewhere—and that, when the shortage of money is severe and long-term rates are higher than they are expected to be in the future, may be very high indeed. Thus the Government has to choose between paying high rates for long periods, by issuing long-term securities at high rates, offering extremely high rates for short periods, and attempting, by means of quantitative restrictions, to prevent borrowers from competing with the Government for the scarce funds. Until recently the Government was relying mainly on the first and third of these methods; since September it has been obliged to have recourse to the second as well.

The two methods, of reducing the supply of money to the income circulation directly through budgetary policy and limiting it indirectly through restricting the supply of money to the capital circulation, are complementary rather than competitive. Budgetary policy has a considerable effect on the aggregate level of national saving; monetary policy seeks to keep investment within the quantity of saving so determined. The need for monetary policy is always present, but the higher the level of saving in relation to the demand for investment the less drastic its enforcement needs to be. Thus the bigger the budget surplus the less drastic will be the monetary policy needed to prevent inflation and (in the absence of a spontaneous increase in the expected profitability of investment) the lower the rate of interest consistent with the absence of inflation. The decision about the size of the budget surplus and the level of interest rates depends partly on considerations about what is technically or politically possible and partly on expectations about the effect of different measures on the incentive to work, but very largely on opinions about how much the standard of living of the present generation should be sacrificed in order to accelerate the rise in real national income and in the standard of living of the next generation.

COSTS, PROFIT MARGINS AND PRICES

If the earlier argument[1] is accepted, it follows that rises in profit margins, wages and salaries are, just as much as the higher prices themselves, merely the symptoms of excess demand and links in the transmission of its effects. Higher profit margins transmit the excess demand only when they lead to the payment of higher wages, salaries and (to a very minor extent) dividends, or when they give rise to increased investment. If higher profits are ploughed back to reserve and used to finance an unchanged investment programme, the effect is to increase the level of aggregate saving without increasing attempted investment and is therefore disinflationary. Thus, for instance, a rise in electricity prices not accompanied by a rise in wages of electricity employees would enable the electricity industry to provide more of the finance needed for its investment programme and relieve the Treasury of part of its responsibility for providing finance for new power stations. The Treasury would then be able to devote part of the budget surplus to repaying government debt, with the effects outlined on p. 99. The effects of a rise in electricity prices and profit margins would thus be identical with those of the imposition of an excise duty on electricity, the proceeds of which were used to increase the budget surplus and repay government debt.

A rise in import prices is not in itself inflationary, in the sense used here, since it does nothing in itself to raise money incomes. If export prices do not rise, the rise in internal prices will be due, not to a rise in money income but to a fall in real income caused by the worsening terms of trade. If, however, import and export prices rise together, money incomes will rise as the result of the rise in export prices (or more strictly of the rise in the domestic component in export prices), while, since the terms of trade are unchanged, real incomes have not improved. The result will therefore be inflationary.

Of the renewed rise of prices since 1953 very little can be attributed to the rise in import prices, From 1953 to 1956 the import price index rose by less than 4 per cent. This by itself would cause a rise in the internal price level of less than 1 per cent., or less than a twelfth of the rise in the price index for consumer expenditure.

WAGES, SALARIES AND COLLECTIVE BARGAINING

Reasons have been given above[1] for believing that the causal factor in the rise in wages and salaries has been, not the demands of trade unions, but the ability of employers to grant them without reducing their demand for labour. Excess demand has kept business so profitable that employers have been in competition with each other for scarce labour, and it is this competition which has forced up labour earnings, with wages following with, in most years, a lengthening time-lag. It is probably true that the very existence of collective bargaining, with its administrative delays, has caused wage rates to rise more slowly than they would have done under similar conditions of excess demand in a free labour market, where there were neither trade unions nor employers' associations. This does not mean, however, that trade union leaders are powerless to influence the rate of rise of wage rates and perhaps also of earnings, for while they cannot increase them they can, in collaboration with employers' associations, bring about a still larger gap between the actual rise and the rise that would have taken place, as the result of excess demand, in a free market. The effect of a " wage freeze ", loyally applied by both employers and employed, could in theory prevent an excess demand for labour from bringing with it an excessive rise in incomes, though it would still involve difficult problems of labour distribution. It could therefore perpetuate a condition of over-full employment without inflation—indeed, inflation would be checked by price control of wages and salaries. In fact, however, it is most unlikely that, without powers of enforcement more severe than would be tolerated in a free country, this solution of the problem could be more than temporary. The continued pressure of excess demand for labour would inevitably cause some employers to find ways of evading the prohibition on paying higher wages, while trade union leaders would find it increasingly difficult to retain the support of their rank and file members. While, therefore, a " wage-freeze " might provide a temporary relief in a period of emergency, it is unlikely to provide a permanent solution of the problem of maintaining full employment without inflation.

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[1] See pp. 94–99.

The time when it is really essential to get the support of the trade union leaders for a policy of moderation in pressing wage demands is when the excess demand has been removed from the system. In any case, the removal of excess demand will bring with it some rise in transitional unemployment. In this country, with its short distances, good communications and well distributed industries, it is believed that the minimum level of unemployment consistent with the absence of excess demand may be much lower than in many other countries—perhaps not more than of the order of 2 per cent. of the working population, or, say, 500,000 persons, most of whom would be unemployed for relatively short periods while moving from one job to another. But if the trade unions used their monopoly power to drive up wages in industries and districts where the labour force would otherwise be expanding, the labour released in industries and districts where it was contracting would remain unemployed. It is not too much to say that while the trade unions, when faced with a determined government, cannot cause inflation, they can determine within wide limits how much unemployment is consistent with its absence.

OTHER RESTRICTIVE PRACTICES

What is true of trade unions is also probably true of other monopolists. During a period of excess demand it is doubtful if they appreciably accelerate the rise in prices, though from the time of Elizabeth I popular opinion has always thought they do. Indeed, it is possible that price agreements, which, like wage agreements, take time to revise, if anything slow down the rise in prices at the cost of causing delays in delivery. In periods of excess demand there is little incentive to limit output, while the function of cartels in keeping high-cost firms in existence side by side with low-cost firms is adequately performed by the excess demand itself. Here also the time when monopoly has the power of doing serious harm is after the excess demand has been removed. With its disappearance, the restriction of output to increase or maintain prices becomes once more attractive, while the sharing of markets again becomes necessary for the survival of high-cost firms. The restriction of output to maintain prices causes unemployment to be higher than would otherwise be necessary, while the maintenance in existence of high-cost firms slows down the rise in real national income.

DISINFLATION AND OUTPUT

The elimination of excess demand will inevitably cause a temporary check to the rise in output, or even a temporary fall. How long this check will last will depend partly on how soon employers, the demand for whose products has fallen, release labour and other resources for alternative uses; but with the disappearance of excess demand there will also almost certainly be a permanent fall in the aggregate number of hours worked throughout the country, as the result partly of the withdrawal from the working population of marginal workers, whom it only just pays to employ even under conditions of excess demand; partly of some increase in unemployment; and partly of the reduction in the hours of overtime worked. In general, the hours no longer worked will be those for which the payments were highest in relation to output; incomes will therefore fall more than output, at least as soon as redundant labour has been discharged, and in spite of the fall in output the net effect of the operation will be disinflationary.

How large and prolonged the check to output will be will depend on how long employers retain redundant labour and how soon the resources so released can find new employment; but given all possible government assistance in facilitating transfer and the co-operation of the trade unions, it is believed that the initial loss does not need to be very large, at least so long as trade in the rest of the world remains active and workers can be absorbed in export occupations. In the longer run, it is believed that the effect may be to accelerate rather than to slow down the rise in production, for the loss of a small number of high-cost working hours is likely to be more than made up by the increased incentive to labour economy and other forms of efficiency induced by more competitive conditions. The recent experience of the motor car industry, which after last year's check to home demand is now producing more cars with less labour, and exporting a higher proportion of them, is a hopeful augury.

The London School of Economics.

A Fallacy in the Interpretation of the Cash Balance Effect[1]

By E. J. Mishan

Today, when the interest in macro-economics has largely shifted from the Olympian controversies of the 'thirties on the determinants of employment and interest toward the techniques involved in the construction of growth and response models, a re-examination of some of these older issues might be thought of as a pastime somewhat wanting in urgency. This is far from being the case however. For if we unwittingly admit a serious ambiguity into what is acknowledged to be a key relationship in macro-economics—and this, in fact, is what has happened—somehow no amount of elaborating the basic relationships with respect to time, or anything else, can get us around this defect. Indeed, until we have this matter settled, we remain exposed to needless controversy and confusion in the future.

The ambiguity in question, and the one which is to exercise us in this paper, exists within the concept of the cash balance effect, a concept not without some topical interest in view of Professor Patinkin's painstaking endeavours to establish a more general classical monetary theory in which the cash balance effect plays a key role.[2] Since the ambiguity springs from a mistaken identification of the cash balance effect proper with the so-called Pigou effect,[3] it will be the task of this paper to demonstrate: (1) that the two effects are quite different; (2) that the so-called Pigou effect, far from being a corrective mechanism in a classical-type monetary system, is operative only within a Keynesian-type monetary system—one in which the rate of interest is stabilised by institutional forces[4]; and (3) that although Patinkin adequately describes the so-called Pigou effect in the article referred to, and in Part I of his book correctly describes the cash balance effect, his macro-economic analysis in Part II of his book is unsatisfactory because, unawares, he sometimes invokes the one effect, sometimes the other.

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[1] I am indebted to my colleagues, G. C. Archibald, K. Klappholz, R. G. Lipsey, and A. W. Phillips, for their many suggestions which have greatly contributed toward the exposition of the ideas contained in this paper.

[2] Don Patinkin, *Money, Interest and Prices* (Row, Peterson & Co., Illinois, 1956). What I call the cash balance effect is referred to, in this book, as the " real-balance effect ". It will become evident in the argument that follows why the term cash balance effect is more appropriate.

[3] In his article, " Price Flexibility and Full Employment ", reprinted in *Readings in Monetary Theory* (American Economic Association, 1951), Patinkin, after describing the Pigou mechanism, says that the argument " is implicit in the classical theorizing on the quantity theory of money ". In his book (*op. cit.*) he goes further. For in a footnote on page 21 he tells us that " The ' real-balance effect ' is identical with what I first called in ' Price Flexibility and Full Employment ' the ' Pigou effect '. This was clearly a bad terminological choice ".

[4] In order to alleviate some of the taxonomic tedium inevitably present in working through four models, I have chosen a main division based on the chief forces assumed to determine the rate of interest. Thus, I distinguish between the ' interest-stabilized monetary systems ' suggested by developments since *The General Theory* and the

I

The first thing to do is to clarify the two concepts in question. The cash balance effect comes into operation when the cash available to the community for transactions purposes—Keynes's M_1—diverges from the amount of cash that the community desires to hold for this purpose. The effect itself takes the form of *attempts* to change the quantity of cash—either (*a*) to add to it by reducing current expenditure below current income, an attempt which in general issues in a decline in aggregate money income, or (*b*) to reduce it by increasing current expenditure above current income, such attempt to reduce cash holdings issuing in general in a rise in aggregate money income. Thus during the operation of the cash balance effect the system is *not* in equilibrium, for the cash balance effect is a mechanism, or a contributory mechanism, by which, following an initial disturbance of the existing level of income, a new equilibrium is brought into being. It is, at this stage, useful to draw an additional distinction, that between (i) a *direct* cash balance effect—one in which, in order to achieve desired cash holdings, attempts to vary current expenditure from current income add directly to, or subtract directly from, aggregate money income—which operates in a classical-type interest-flexible system, and (ii) an *indirect* cash balance effect—one in which such attempts affect aggregate money income indirectly through their influence on the (imperfectly stabilized) rate of interest in so far as movements in the latter change the current levels of saving and investment—which operates in an interest-stabilized monetary system. We now turn our attention to the other concept.

'interest-flexible monetary systems' commonly associated with the Neo-Classical Cash Balance approach.

My grounds for this distinction are those indicated at some length in a previous paper, "Toward a General Theory of Price, Income, and Money", *Journal of Political Economy*, 1952. Briefly, if we had to put our finger on the one essential feature that differentiates post-Keynesian macro-economic models from Neo-Classical, or Cash Balance, models, that feature would surely be found in the postulated determinants of the rate of interest.

In the interest-flexible, or cash balance, models, bond prices are determined, like the general class of commodities, by the current demand for and supply of new issues; which is to say, in the case of bonds, solely by current saving and investment. Only in an interest-flexible monetary system may we talk of 'the dog wagging the tail'—the dog being the over-all activity of the economy as measured by aggregate money income, the tail being the bond market. In contrast, monetary systems in which the rate of interest is stabilized by a Keynesian Liquidity Preference function are systems of which it may be said that 'the tail wags the dog'. In such systems an autonomous movement of the schedule of current investment or saving induces a movement in all other markets taken together—in aggregate money income in fact, a movement which is necessary to equate current saving and investment, and therefore to restore equilibrium in the bond market for new issues.

Despite the title of Professor Pigou's famous paper, " The Classical Stationary State ",[1] the relationship introduced therein was designed to reveal the *modus operandi* by which, given a Keynesian underemployment situation, a declining price level might eventually restore aggregate real income and full employment. A more suitable nomenclature for the so-called Pigou effect, and one which makes its nature more apparent, is the asset-expenditure effect.[2] In substance the relationship is simple: a rise in the net value of the assets of the community entails a rise in its propensity to spend. The subsequent discussion of the asset-expenditure effect turned about two main themes. One was the mechanism by which assets change in value, which led to a review of the kind of assets which might be taken to impart a net effect of the sort necessary to actuate the asset-expenditure mechanism. The second concerned the ways in which this corrective mechanism might be wrecked by dynamic factors, in particular by expectations formed from observing a continuous rise, or fall, in the level of prices. The course of this controversy, however, will not concern us here. We shall attend only to the distinction between the two relationships described and their respective roles in monetary systems, a matter which, to the best of my knowledge, has not been dealt with by any of the participants.

In drawing a clear distinction between these two relationships and their respective implications it will be inevitable, in order to guard against misunderstanding, to cover some fairly familiar ground. An interest-flexible monetary system and an interest-stabilized monetary system will be set up in both of which the role of the cash balance effect will be briefly examined. Following this, the role of the asset-expenditure effect in both these systems will be subjected to scrutiny. In all cases we will observe the different consequences, if any, which flow from the assumption of fixed money wage-rates in contrast to flexible money wage-rates.

II

Let us define a classical, or interest-flexible, monetary system, with two alternative labour markets, by the following group of equations:

$$\text{I}\begin{cases} C=C\,(Y,r) \;\ldots\ldots(1)\\ I=I\,(Y,r) \;\ldots\ldots\ldots(2)\\ S=Y-C \;\ldots\ldots\ldots(3)\\ S=I \;\ldots\ldots\ldots\ldots(4)\end{cases} \qquad \text{II}\begin{cases} M_d=K.Y \;\ldots\ldots(5)\\ M_s=M_o \;\ldots\ldots(6)\\ M_d=M_s \;\ldots\ldots(7)\end{cases}$$

$$\text{III}\begin{cases} N_d=N\,(W,Y) \;\ldots\ldots\;(8)\\ N_s=N_o \;\ldots\ldots\ldots\ldots\;(9)\\ N_d=N_s \;\ldots\ldots\ldots\ldots(10)\end{cases} \qquad \text{III}'\begin{cases} N_d=N\,(W,Y)\ldots\ldots\ldots(8)\\ W=W_o\ldots\ldots\ldots\ldots(9')\\ N_d=N_s \;\ldots\ldots\ldots\ldots(10)\end{cases}$$

$$\text{IV}\begin{cases} Y=P.T \;\ldots\ldots(11)\\ T=h.N_s\ldots\ldots(12)\end{cases}$$

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[1] *Economic Journal*, LIII, 1943.

[2] Lloyd Metzler, in his " Wealth, Saving, and the Rate of Interest ", *Journal of Political Economy*, 1951, adopts the term *wealth-saving relationship*, indicating that the influence of a change in the real value of the community's net assets is, in Metzler's model, confined to current saving. Though in general a change in the value of net

The letters stand for the following variables: C, aggregate consumption expenditure; I, aggregate investment expenditure; S, aggregate saving; Y, aggregate income. These magnitudes are current flows and are measured in *money* terms. M_d represents the aggregate quantity of money demanded by the community for transactions purposes; M_s represents the amount of money supplied; N_d, aggregate demand for labour; N_s, aggregate supply of labour; P, the general price-level; T, real output; r, the rate of interest. M_0 is the quantity of money in the economy; N_0, the total quantity of labour available in the economy; W_0, the fixed money wage-rate. K is a constant—the desired cash-income ratio of the community, and h is a constant—the average (equals marginal) physical product of labour.

The group of equations in II, the " money market ", indicate that aggregate money income is determined by monetary forces alone. Regardless, that is, of the values taken on by the other variables, given K and M_0, aggregate money income is determined. It follows that in any adjustment process of aggregate money income in this interest-flexible system the *direct* cash balance effect alone is in operation.

The equations grouped together in I, the " commodity market ", act to ensure that, *in equilibrium*, the entire money income—as determined in II—issues in current expenditure. This process is a consequence of what we may call an " equilibrating " rate of interest—a rate of interest which, by itself, equates current saving to current investment, thereby channeling into current investment all income that is not used in current consumption.

As explained above, we assume a choice of two " labour markets ". The group of equations in III represent the case where the supply of labour with respect to the money wage-rate is of zero elasticity, a situation we may refer to briefly as *flexible* money wage-rates. Equilibrium with this kind of labour market therefore entails full employment of the available supply of labour, N_0. III′ differs from III only in that equation (9′) substitutes for (9). III′ represents the case of a perfectly elastic supply curve of labour with respect to the money wage-rate, W_0, or, in brief, the case of *fixed* money wage-rates. Equilibrium of the system with the III′ labour market is consistent with various levels of employment and output.[1]

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assets may be considered also to influence the schedule of current investment, provided that the schedule increases when assets increase (as is assumed here), no difference to the conclusions follow from adopting the more general form which includes the effect on both saving and investment.

[1] Clearly the III′ labour market assumption is plausible only for a range of employment bounded by two limits, a minimum level of employment and a maximum, or " full employment ", level, at the borders of which money wage-rates tend to become flexible downward and upward respectively. The shorter the time period under consideration, the wider may we expect this range of employment to be.

It may be remarked that nothing is gained in the exposition, or lost in the essentials of the argument, by substituting instead of the horizontal supply curve in money terms the more conventional upward sloping supply curve of labour in money terms.

The two equations in IV are inserted for completeness and are of this simple form in order to free our minds of irrelevant elaboration. In sum, they show that, when employment is fixed at N_o as in III, the price-level varies directly with aggregate money income, while if, in contrast, the money wage-rate is fixed at W_o, as in III′, and hence—because of the constant physical returns to labour denoted by equation (12)—the price-level also is fixed, employment varies directly with the level of money income.

Taking our choice of III or III′, the above set of equations form an equilibrium system where the magnitudes of all the variables are simultaneously determined. There are twelve equations and twelve undetermined variables in the system and, provided, of course, that the twelve equations are independent and consistent with one another, a solution is possible.

In order to proceed to illustrate the process of adjustment, following an autonomous change in K or M_o, it will be an advantage to substitute for equations (1) and (2) the more general equations which obtain whether the system is in equilibrium or not:

$$C=C_1\,(Y, r, [M_s-M_d]) \qquad \text{(1.1)}$$

$$I=I_1\,(Y, r, [M_s-M_d]) \qquad \text{(1.2)}$$

It follows from our description of the direct cash balance effect that according as $M_s \gtreqless M_d$, $E \gtreqless Y$; where E is aggregate money expenditure equal in value to current purchases of consumer goods and bonds.

If, for example, some equilibrium situation is disturbed by an autonomous increase in the supply of money, so that, initially, $M_s > M_d$, then, initially, $E > Y$. Aggregate money income then rises in response to this additional expenditure thereby raising M_d (granted K remains unchanged). E ceases to exceed Y when M_d has risen sufficiently to equal M_s. That is to say, *the process of adjustment being over, the expression* $[M_s-M_d]$ *is equal to zero* and may be removed from equations (1.1) and (1.2) above to give us equations (1) and (2) again in a system that yields an equilibrium solution for all the variables.

It is particularly to be observed that we have been dealing only with *money* magnitudes, and the process of adjustment just described adequately comprehends the cases of both flexible and fixed money wage-rates. With flexible money wage-rates, employment remains unchanged but prices rise directly with aggregate money income. If, on the other hand, we adopt the assumption of fixed money wage-rates, prices remain unchanged but employment increases directly with aggregate money income. In other words, *a change in the price-level is not necessary for the operation of the cash balance effect; a change in the level of money income is sufficient.* We are further justified in employing Y alone for the cash balance effect, and not Y/P as is Patinkin's practice, since Y/P is necessary, as we shall see later, for introducing the asset-expenditure effect into the monetary system.

We may conclude that in a classical, or interest-flexible, monetary system K and M_0 between them determine the level of money income. In the event of flexible money wage-rates, an autonomous change in K or M_0 directly affects the level of prices through the process described above as the cash balance effect. Whereas in the event that money wage-rates are fixed an autonomous change in K or M_0 directly affects the level of employment through the cash balance effect.

III

In order now to switch to an interest-stabilized monetary system we need only change a single equation. Instead of (5) we now write $M_d = M(Y, r)$......(5′). As before M_d is directly related to Y but, in addition, it is now inversely related to r.

The prime consequence of this modification is that, on the one hand, in the I set of equations the rate of interest is no longer free to equate the current saving and investment schedules since it is now related also to the demand and supply of money. On the other hand, K and M_0 between them are no longer the sole determinants of the level of money income. As a result, any adjustment of the level of money income depends only partly on the cash balance effect, which is now *indirect*: for the rest it depends on the multiplier—the Keynesian " *income*-expenditure " effect. And the more stabilized is the rate of interest the greater are the movements in the level of money income necessary to restore equilibrium.

Furthermore, as a result of the interaction of the two markets, the money market and the commodity market, a disturbance of the equilibrium of aggregate money income may start in either market. An autonomous shift in the consumption function or in the investment function, or an autonomous change in K or M_0 requires adjustment to take place in both markets simultaneously until once more the level of money income and the rate of interest are in equilibrium. And again, just as in the interest-flexible monetary system, a movement in aggregate money income will be the concomitant of a proportional movement in the price-level when money wage-rates are flexible, or of a proportional movement in the level of employment when money wage-rates are rigid.

Turning to the process of adjustment, let us assume that an initial equilibrium of the system has been disturbed by an upward shift in the level of current investment. Owing to some degree of stabilization of the rate of interest by the " money market " the response of the rate of interest falls short of that necessary to bring current investment and saving into equilibrium again. The consequent excess current investment engenders a rise in aggregate money income and, because of the additional cash requirements of a higher level of money income, an upward pressure on the rate of interest—the *indirect* cash balance effect. On the one hand then, the rising level of money income shifts the saving and investment schedules. On the other hand, the *indirect* cash balance effect pushes up the rate of interest. Provided the system is stable—the saving schedule more responsive to income than the investment schedule—these adjusting mechanisms bring the system into equilibrium once again.[1]

To sum up on the cash balance effect in both types of monetary system. In the interest-flexible monetary system, where the rate of interest has the sole function of equating current saving and investment, K and M_o between them uniquely determine the level of money income. According as we posit fixed or flexible money wage-rates real income and employment or the price-level are determined respectively. An autonomous change of K or of M_o, such that initially $M_s \neq M_d$, brings into operation the direct cash balance effect until $M_s = M_d$, and equilibrium of the system is restored.

In the interest-stabilized system, on the other hand, both the level of money income and the rate of interest are simultaneously determined in the " commodity market " and the " money market ". As for the adjustment process, any autonomous change in K, in " Liquidity Preference ", or in M_o, or alternatively any autonomous change in the propensities to save or to invest, or in exogenous investment, will disturb the equilibrium level of money income and of interest and bring into operation the *indirect* cash balance effect and also the Keynesian Multiplier, or income-expenditure, effect acting directly on the saving and investment schedules. Finally, as in all monetary systems, whether the necessary adjustment in aggregate money income is to be measured in terms of price changes or employment changes will depend on whether money wage-rates are taken as flexible or fixed.[2]

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[1] It should be recalled that the autonomous shift in the investment schedule was in money terms and therefore with flexible money wage-rates a rising level of money income raises only the schedule of *money* saving, and not—if we rule out money illusion—real saving. Hence, if producers persist in their attempt to raise the level of real investment, money income, and therefore prices, will continue to rise and to absorb cash for transactions purposes until the rate of interest reaches a level which brings into equilibrium the unchanged real saving schedule with the new and increased real investment schedule.

The extreme case in which $r = r_o$ yields us the Wicksellian system: an autonomous shift in real investment entails an indefinite rise or fall in the level of money income, for the *indirect* cash balance effect has no influence whatever on the rate of interest which is, by assumption, completely stabilized.

[2] It follows from all we have said that (i) if *either* the rate of interest *or* money wage-rates are properly flexible—in the senses used here—then the rate of interest is, in equilibrium, determined only in the ' commodity market ' and aggregate money income in the ' money market ', provided there is no money illusion. (ii) If *both* the rate of interest is completely stabilized, and money wage-rates are rigid, the reverse may be taken to hold true. In the event that money wage-rates are flexible, the slope of the Liquidity Preference function—if we choose to introduce one into our system—influences only the degree of income adjustment required to restore equilibrium. It has no effect on the level of the rate of interest which is determined uniquely—in the absence of any asset-expenditure effect—by the full employment schedules of real saving and investment. In the more usual macro-economic model, in which money wage-rates are fixed and the rate of

IV

In order to introduce the asset-expenditure effects we substitute for equations (1) and (2) respectively, the following equations:

$$C=C_i\ (Y,\ r,\ M_s/P)\ldots\ldots(1'),\ \text{and}\ I=I_i\ (Y,\ r,\ M_s/P)\ldots\ldots(2')$$

It is necessary that M_s be divided by P for we now wish to introduce into these systems, as an influence on current consumption and investment, the net value, in real terms, of the community's assets of which money balances are the only form chosen in this paper.[1] Dividing M_s by P indicates clearly that a decline in aggregate money income which does not, in some degree, reduce the level of prices does not raise the net value of the community's cash holdings and does not therefore invoke the asset-expenditure mechanism.

It is essential to notice that the variable M_s/P in (1′) and (2′) forms part of an equilibrium system of equations, whether of the interest-flexible or interest-stabilized variety, for its impact is on the level of current expenditure—on the schedules of current consumption and current investment which obtain when the system is in equilibrium. In contrast, the equations indicative of the cash balance effect, (1.1) and (1.2), take on values for the M term only when the system is out of equilibrium. In equilibrium, as we have pointed out, $[M_s–M_d]$ is zero.[2]

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interest is *imperfectly* stabilized, the rate of interest and the level of money, and real, income are simultaneously determined in both the 'money market' and the 'commodity market'.

[1] We may assume the existence of commodity money, say gold, as the only circulating medium of exchange in order to evade the problems associated with the creditor-debtor relationship. In these circumstances a decline in the price-level unambiguously raises in real terms the net money holdings of the community.

In general, of course, one should write A/P, where A stands for the net asset holdings of the community, including money. But concentrating on the M/P component illustrates the essential mechanism plainly enough and is, in addition, revealing when we come to speculate on the source of the mistaken identity of the asset-expenditure effect.

[2] This simple proposition can endure repetition, for it is close to the hub of a good deal of controversy concerning the "neutrality" of money in a classical system. As a recent example of this controversy we may glance at a "refutation" of a statement by Modigliani in a recent article by F. H. Hahn.

In his "Liquidity Preference and the Theory of Interest and Money" (reprinted in the *Readings in Monetary Theory*, A.E.A., 1951) Modigliani's chief conclusions include the proposition that "under 'static' assumptions and 'flexible' wages, *the rate of interest and the level of employment do not depend on the quantity of money*" (p. 218, his italics).

F. H. Hahn in his article, "The Rate of Interest and General Equilibrium Analysis" (*Economic Journal*, 1955), sides with Patinkin and takes exception to the above statement since it implies neutral money. His attempt to refute Modigliani's conclusion has value as an example of popular misapprehension.

"Let us assume [he writes on page 56] that relative prices and the interest rate are such that the total demand for goods and services exceeds their total supply—a situation perfectly possible as long as we do not assume Say's law. If Mr. Modigliani is right, we can now find some level of absolute prices which will make the demand for money equal to its supply. Choosing such an absolute price has, by assumption, no effect on the total demand or supply of goods and services. But we have now arrived at a position where the total demand for goods and services exceeds their

Since we have modified our equations systems by inserting M_s/P into the consumption and investment functions, the mechanism of the cash balance effect when this modified system is out of equilibrium can be indicated by writing instead of equations (1.1) and (1.2)

$$C=C_2\ (Y,\ r,\ [M_s-M_d],\ M_s/P)\ldots\ldots(2.1')$$

$$I=I_2\ (Y,\ r,\ [M_s-M_d],\ M_s/P)\ldots\ldots(2.2')$$

In these equations, then, we have the ingredients of *both* the cash balance effect and the asset-expenditure effect. The cash balance effect functions only so long as $M_s \neq M_d$. Thus, whereas in equilibrium $[M_s-M_d]$ vanishes from the equations, M_s/P remains in the equations, as a determinant of the level of consumption and of investment expenditure, whether the system is in equilibrium or not. None the less this asset-expenditure relation comes into action as the asset-expenditure *effect* only if the system is out of equilibrium and if M_s/P changes as a consequence of a changing price-level.

Let us now enquire what difference the asset-expenditure effect makes to the equilibrating mechanism of the interest-flexible and interest-stabilized monetary systems respectively.

In the former type of system, irrespective of flexible or fixed money wage-rates, it makes this difference: that, since a rise in the net value of the community's money holdings shifts the consumption and investment schedules to the right, a fall in P raises the equilibrium rate of interest and alters, in general,[1] the equilibrium level of current saving and investment. There are no repercussions at all on the level of money income.

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supply, but the demand for money is equal to the stock of money, and this clearly contradicts Walras's identity described in the previous paragraph."

Hahn's error arises from a confusion between the cash balance effect which requires a *disequilibrium* of the monetary system for its operation, and the cash balance equation which obtains only in equilibrium.

To illustrate in more detail, Hahn introduces us to a disequilibrium situation, one of excess current demand and, consequently, one generating an upward tendency in the economy. When prices rise sufficiently full equilibrium is restored: the total demand for money is equal to the total stock of money, and the total demand for goods is equal to the total supply of goods. And it is quite in order that a finite rise in the price-level eliminates the initial excess demand for goods, for the initial position is one of disequilibrium—one in which $M_s > M_d$. Thus, excess demand for goods in *disequilibrium* is quite consistent with the further proposition that in the newly established equilibrium real demand for goods is no different from that which existed in the old equilibrium. 'Full employment', at N_0, still obtains in both equilibrium positions (provided there is no money illusion). Only a higher price level in the new equilibrium distinguishes it from the old. The M-term is the same in both equilibria—zero.

It should be clear from the argument in the text that this result holds for flexible money wage-rates even if we introduce a "Liquidity Preference" function and, therefore, rely for adjustment on the *indirect* cash balance effect.

[1] If we postulated instead an asset-consumption relation (or asset-saving relation) as does Metzler, *loc. cit.*, then, of course, current saving and investment would decline along with a rise in the rate of interest as the price level fell. It is when both consumption and investment expenditure respond to the net value of assets that the resulting direction of the change in saving and investment is uncertain.

This is the essence of the matter, for if we are introduced to a disequilibrium situation in which aggregate money income and the level of prices are falling there is nothing in the operation of the asset-expenditure effect by itself to inhibit this movement. In the absence of the cash balance effect, involuntary underemployment would remain and the level of money income and prices would fall indefinitely. Put otherwise, the asset-expenditure relation cannot determine the level of money income in an interest-flexible system; this is a role which is reserved for the " money market " alone—for K and M_0.

We conclude that in an interest-flexible monetary system the asset-expenditure effect, though it influences the rate of interest and the rates of current saving and investment, has no part to play in maintaining or restoring an equilibrium level of money income.

V

It is otherwise in an interest-stabilized economy for the simple reason that, with the rate of interest stabilized in some degree, shifts of the consumption and investment functions do entail movements in aggregate money income.

However, the asset-expenditure effect, as an automatic corrective mechanism, does require a decline in the price-level to bring it into operation and therefore if money wage-rates remain rigid in the face of a decline in money income and, therefore, of employment the effect in question cannot arise. Consider, therefore, an economy in which money wage-rates are flexible. A disturbance to the equilibrium level of money income may arise in the commodity market or in the money market, but since it makes no difference to the corrective mechanism involved we can suppose that the system is initially pushed out of equilibrium by an autonomous reduction in current investment. The resulting fall in the price-level, and hence rise in M_s/P, which accompanies the fall in aggregate money income, shifts the consumption and investment schedules to the right and therefore contributes to that extent to the restoration of equilibrium in the system.[1]

The *indirect* cash balance effect will, in these circumstances, also be at work. But even if it were held in abeyance—as, in effect, it would be if the rate of interest were completely stabilized—the asset-expenditure effect would tend to restore equilibrium. For, given flexible money wage-rates, the price level will decline so long as involuntary unemployment remains,[1] thereby shifting the investment and consumption functions.[2]

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[1] The introduction of the asset-expenditure relation modifies the conclusion we reached when dealing with the cash balance equations earlier on: that in a flexible wage-rate economy with an imperfectly stabilized rate of interest the equilibrium rate of interest is determined in the commodity market—that is, by the ' full employment ' schedules of saving and investment—granted there is no money illusion. For with the introduction of the asset-expenditure relation, the investment and saving schedules themselves shift as the price-level changes, and hence the underlying equilibrium rate of interest is altered.

We may remind ourselves at this final stage of the analysis that we have been going through these exercises using models based on either fixed or flexible money wage-rates. The asset-expenditure effect is less limited than it may at first appear if we are willing to allow that, given enough time, the existence of involuntary unemployment will lead *eventually* to some decline in the price-level and therefore eventually to some corrective tendency on the part of the asset-expenditure mechanism.

To sum up on the asset-expenditure effect. Considered as an additional mechanism of income adjustment the asset-expenditure effect is inoperative in monetary systems in which the rate of interest directly equates current saving and investment, as in the classical-type models used here. It is also inoperative in a system in which money wage-rates are fixed since, unlike the cash balance effect, it requires for its action a movement in the price-level and not merely a movement in the level of money income. Expressed in a positive form: in order for the asset-expenditure effect to act as an income-equilibrating mechanism two conditions must be fulfilled. First, money wage-rates must be flexible in some degree over the period in question—else the net value of money holdings does not alter. Second, the rate of interest must be stabilized in some degree (the demand to hold money with respect to the rate of interest must be of greater than zero elasticity)—else asset-induced shifts in the investment and consumption functions affect the interest rate but not aggregate money income. In contrast, the cash balance effect operates whether or not these conditions are fulfilled.

Hence, far from being similar to the cash balance effect which is the sole automatic corrective mechanism of aggregate money income in a classical interest-flexible system, the asset-expenditure effect has, in such a system, no effect whatsoever on the level of aggregate income. Only in a system in which the rate of interest is in some degree stabilized may the asset-expenditure effect assume the role of an income-equilibrating mechanism.

VI

Though Patinkin believed that the two effects we have distinguished were identical it might have been hoped that a close study of Part II of his book would disclose whether he was thinking consistently of the cash balance effect proper or of the asset-expenditure effect. A perusal of the text, however, suggests that in some instances he was thinking of the latter,[1] in other instances of the former.[2] And his mathematical notation, far from pointing up a distinction between the two effects, lent itself nicely to the same ambiguity.

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[1] If we include the public debt among the community's net assets the fall in the price-level has a more powerful effect. And if, further, we allow that the rate of interest falls somewhat as the real value of money holdings rises then, in addition to the *indirect* cash balance effect, the equilibrating tendencies of the system are reinforced by an additional rise in the net value of assets as a result of the increase in the market value of government bonds.

[2] Without changing the form of our equations it should be manifest that the consumption and investment functions increase in *real* terms from the consideration that in money terms they decline in smaller proportion than the price-level as a consequence of their simultaneous shift to the right.

This may be illustrated by substituting real magnitudes for the money magnitudes of the variables in our first two equations. We have shown that this is not necessary if we are dealing only with the cash balance relation and that if we wish to indicate the presence of the asset-expenditure relation we need only insert the real variable M_s/P into the equations. Nevertheless, it will facilitate comparison with Patinkin's expressions to do so. We shall indicate the real variables by an asterisk; thus, C^* stands for C/P, and similarly for I^*, Y^*, and M^*.

Our equations (1′) and (2′), containing the asset-expenditure relation, can therefore be re-written:

$$C^* = C_t\,(Y^*, r, M^*_s) \text{ and } I^* = I_t\,(Y^*, r, M^*_s)$$

By adding C^* and I^* to give us E^*, aggregate real expenditure, we can write $E^* = E_2\,(Y^*, r, M^*_s) \ldots\ldots (e.2)$

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[1] For instance, on page 155 (*op. cit.*), in describing his Fig. 18, Patinkin tells us that the negative slope of his *CC* curve is a locus of all pairs of *equilibrium* values of the rate of interest and the price-level. " Clearly, [he says] the higher the given interest rate, the greater the real-balance effect necessary to offset its depressing effect on demand, and the lower, therefore, the price level necessary to generate this effect."

We should, perhaps, instead of " real-balance effect ", write " the real supply of cash ", since the ' real ' or cash balance *effect* is, on our interpretation, the mechanism of adjustment. Nevertheless, the meaning of the statement is clear: if we wish to maintain aggregate money income in equilibrium at its existing level, and then arbitrarily raise the rate of interest, we must ' compensate ' by an increase in the net value of cash holdings—that is, given M_s by a decline in P, the price-level. But this makes sense only if we are thinking in terms of the asset-expenditure effect in a system having a stabilized rate of interest. In such a system, the initial gap of excess saving, following our arbitrary rise in the rate of interest, may be closed by a shift of the saving and investment schedules by increasing the real value of assets by a finite amount—by a finite fall in P.

The cash balance relation, on the other hand, does not accommodate Patinkin's argument for, there, a finite reduction of the price-level entails an attempt to dispose of a finite amount of the consequent rise in the real value of cash holdings. This can be effective as an increase in aggregate expenditure only over a *finite* period—say, the ' week ' to which Patinkin confines himself in part I of his book. But, clearly, a *once-for-all* attempt to ' dishoard ' cash by the community has no power to maintain the *flow*, or *level*, or money income in the face of an excess *rate* of saving. Indeed, to offset the effect on the level of money income of an excess rate of saving requires a determinate *rate of fall* in the price-level—at least, this is so if we confine ourselves to the cash balance effect.

[2] For instance, on page 159 (*op. cit.*), when describing the after-effects of a doubling of the quantity of money by the government (even if the additional money is distributed among the community as transfer payments) Patinkin speaks of this additional money pushing up the demand curve for goods and also, therefore, the price-level, in this way reducing the real value of cash balances, a process which continues until prices have doubled and the real position has been restored. This description provides a clear example of what we have called the *direct* cash balance effect—an initial excess supply of money raising aggregate money income until the additional demand for money for transactions purposes becomes equal to the additional supply.

It should be observed that this equation, (e.2), is the same expression as that adopted by Patinkin for his illustration of the cash balance effect.[1]

Turning now to our equations (1.1) and (1.2) indicative of the cash balance mechanism, these also can be re-written in real terms, and C^* and I^* added together to give us

$$E^*=E_1\,(Y^*, r, [M^*_s - M^*_d])\ldots\ldots(e.1)$$

Leaving (e.1) in this partly specific form we have a useful reminder of the nature of the cash balance effect and of the distinction between this equation and the one we used above to express the asset-expenditure relation, (e.2). But if, instead, we choose to cast the expression (e.1) in its most general form, this distinction is lost. For by substituting $K.Y^*$ for M^*_d, (e.1) may be written

$$E^*=E_1\,(Y^*, r, [M^*_s - K.Y^*])$$

whence $E^*=F\,(Y^*, r, M^*_s)\ldots\ldots(e.12)$

Hence, equation (e.12) which originally contained only the cash balance ingredient is now, in this general form, identical with (e.2) which contains only the asset-expenditure ingredient. Indeed, it must be obvious that we can have the ingredients of *both* these effects in yet the same general form, (e.12), which is used by Patinkin. It looks as if we have here an example of the use of mathematical notation which served to conceal, rather than to reveal, an essential distinction.[2]

The London School of Economics.

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[1] For example, see page 131 (*op. cit.*). Though he writes $E=F\,(Y, r, M_0/P)$ Patinkin makes it plain in the text that E represents real expenditure—real investment expenditure plus real consumption expenditure—and that Y represents real aggregate income.

[2] For other, and more general, criticisms of Patinkin's book the reader is referred to a forthcoming review article by Archibald & Lipsey in the *Review of Economic Studies.*

A Dynamic Analysis of Structural Change in a Leontief Model[1]

By Michio Morishima

1. The so-called Leontief model, originally static in character, is usually dynamized by introducing consumption- and production-lags, accelerators, growth of final demand, structural changes due to technological invention, and so on. In this article we are not concerned with consumption-lags, the acceleration principle, or the growth of final demand. Instead, we shall concentrate on structural changes due to technological invention on the assumption that the period of production of each good is one unit time. Since my analysis is dynamic, it will depart from the comparative statics approach to structural change as used by Professors Leontief, Simon and Rasmussen.[2] The original version of this paper was written in mathematical form, but following the editor's suggestion, I have removed the armour of mathematics and attempted to explain my problems in plain language. All the propositions stated in the theorems below can be rigorously proved, but the mathematical demonstration is omitted.

2. The Leontief system consists of two sets of equations, the first describing the balance of input and output for all products, and the second the equality of costs and prices. Solving the first and the second sets, we obtain equilibrium outputs and equilibrium prices respectively.

We assume that a national economy is divided into n industries each of which produces a single commodity. It is further assumed that the i-th industry can choose between m_i different manufacturing processes or activities in producing commodity i. An activity is defined by a vector (with $n + 1$ components) stating the inputs of n commodities and of labour per unit output ;[3] labour is indispensable, and

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[1] I should like to express my sincere gratitude to Professor R. G. D. Allen of the London School of Economics and to Dr. F. Seton of Nuffield College who read my earlier draft and made valuable suggestions. Dr. Seton has also helped in bringing my English into readable form. I could not have rewritten my original version in plain language without his help. But, needless to say, to the author alone belongs the responsibility for any errors which may be found in this paper.

[2] Leontief, W., " Structural Change ", *Studies in the Structure of the American Economy*, ed. by Leontief, 1953; Simon, H. A., " Effects of Technological Change in a Leontief Model ", *Activity Analysis of Production and Allocation*, ed. by Koopmans, T. C., 1951; and Rasmussen, P. N., *Studies in Inter-Sectoral Relations*, 1956.

[3] Let $a_{ji}^{(k)}$ be the quantity of good j consumed per unit of output i by the k-th activity of industry i, and $b_i^{(k)}$ the labour input coefficient of that activity; then we have activity vectors of industry i:

$$\left[a_{1i}^{(k)}, a_{2i}^{(k)}, \ldots\ldots, a_{ni}^{(k)}, b_i^{(k)}\right] \; (k = 1, 2, \ldots., m_i;\; i = 1, 2, \ldots, n).$$

each activity has constant returns to scale with fixed input coefficients. The model could be stated in terms of " gross " output, but for simplicity, we shall use " net " outputs only; hence the intra-sectoral input coefficients of all activities vanish. Since the i-th industry can adopt any one of m_i activities, there are $m_1 \times m_2 \times \ldots \times m_n$ possible Leontief systems. To each of them will correspond a different set of equilibrium prices, which we shall call the price-set " proper " to the system.[1] Conversely, the system of activities resulting in the given price-set, and each of the activities within it, will be described as " proper " to that set. In what follows we shall assume that the range of choices open to our industries includes at least one Leontief system which is " economically relevant " in the sense of having a non-negative set of prices proper to it.

It can be proved that among the possible Leontief systems which are economically relevant there will always exist one system with an equilibrium price-set not greater (in the sense of vector comparison)[2] than any other equilibrium set (Theorem 1). We shall call this the minimal set. It can also be proved that a set of equilibrium prices is minimal if and only if there are no available activities less expensive at that set of prices than the activities " proper " to it[3] (Theorem 2). Theorems 1 and 2 may be illustrated by Figure 1 where the co-ordinates represent the prices (or unit costs) of product 1 and 2 respectively. Let activities I_1, II_1, III_1 be available to the first industry and activities I_2, II_2, III_2 available to the second. Since these activities are *net* activities, the unit cost of the first product depends on the price of the second only, and conversely. The solid lines (I_1, II_1 and III_1) stand for the unit cost of the first product when produced by activities I_1, II_1 and III_1. In the same way the dotted lines (I_2, II_2 and III_2) trace the unit cost of the second product. Possible sets of non-negative equilibrium prices are represented by $p^{(a)}, p^{(b)}, \ldots., p^{(e)}$, the points of intersection of all pairs of cost lines in the non-negative quadrant; and $p^{(e)}$

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[1] In this paper the *numéraire* is labour, i.e., p_i denotes the price of good i in terms of labour.

[2] A vector is not greater than another if no element of the former is greater than the corresponding element of the latter.

[3] *The set of minimal equilibrium prices is a maximal solution of the following linear programming problem*:

$$0 \leqq p_i \leqq a_{1i}^{(k)} p_1 + a_{2i}^{(k)} p_2 + \ldots\ldots + a_{ni}^{(k)} p_n + b_i^{(k)}$$
$$(k = 1, 2, \ldots, m_i;\ i = 1, 2, \ldots., n)$$
$$\max = p_1 + p_2 + \ldots\ldots + p_n,$$

and a minimal solution of the linear programming problem:

$$p_i \geqq \min_{k = 1, 2, \ldots., m_i} \left[a_{1i}^{(k)} p_1 + a_{2i}^{(k)} p_2 + \ldots\ldots + a_{ni}^{(k)} p_n + b_i^{(k)} \right]$$
$$(i = 1, 2, \ldots., n)$$
$$\min = p_1 + p_2 + \ldots\ldots + p_n.$$

Points in the dotted (or shaded) area (Figure 1) are " feasible " solutions of the first (or second) linear programming problem. Programming calculations by the simplex method give a series of price vectors $p^{*}, p^{**}, p^{***}, \ldots$, which reaches the minimal set, $p^{(e)}$, at the end of the calculations. Note, however, that this series has no relation to the time-path of actual prices which is involved in Theorem 3.

gives minimal equilibrium prices. At $p^{(e)}$, activities I_1 and III_1 are not less expensive than II_1, the activity " proper " to $p^{(e)}$ in the first industry. Nor are I_2 and III_2 less expensive than II_2, the activity " proper " to $p^{(e)}$ in the second industry.

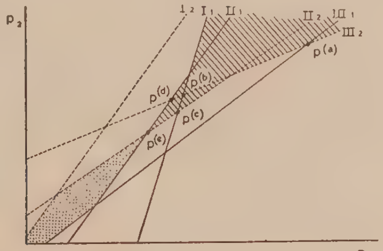

FIG. 1

Now assume that each industry selects the cheapest activity from among those available to it and that it sells its output at cost price; let the production period for each output be one unit time. The given prices of period t determine the cost-minimizing activities which will be adopted in that period, and the products will be sold in the next period $(t + 1)$ at the minimal cost arising in this way. Thus we have dynamic equations relating the prices of period t to those of period $t + 1$ and a time path of prices. We can show that any path starting from an arbitrary initial position of prices eventually reaches the point of the minimal equilibrium prices, i.e., the point has the stability of the first kind (in the sense of Samuelson)[1] both in the large and in the small (Theorem 3). On the way to this point, prices change, and therefore the cheapest activity of each industry may change from period to period, so that the Leontief input coefficients may also change. Figure 2 illustrates the convergence of prices. When the initial point of prices is $p(0)$, activity III_1 of the first industry and I_2 of the second industry are selected as the cheapest in period 0, II_1 and I_2 in period 1, and II_1 and II_2 in period 2 and all subsequent periods. When the initial point of prices is $p'(0)$, the pairs of activities selected will be I_1 and III_2 in period 0, I_1 and II_2 in period 1, and II_1 and II_2 thereafter.

If the level of all outputs is given, a Leontief system determines the final outputs and labour requirement of the whole economy. Let us call a Leontief system " efficient " if there exists no possible Leontief system which improves on it in the sense that the final output of one commodity can be increased without diminishing that of another or increasing labour requirement.[2] Activities which are components of efficient Leontief systems will be called efficient activities. We can prove that the Leontief system proper to the minimal equilibrium prices is an efficient system (Theorem 4). By assuming that each industry at once selects the efficient activity from among its available activities, Professors Samuelson, Koopmans, Arrow and Georgescu-Roegen have proved the so-called Substitution Theorem.[1] In our model, however, it will take some time for the matrix of input coefficients to become efficient. If the initial point of prices is $p(0)$ or $p'(0)$ (Figure 2), the matrix is not efficient until period 2. Thus the Substitution Theorem holds only at the later stage of the process of convergence of $p(t)$. It does not hold at the earlier stage.

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[1] Samuelson, P.A., *Foundations of Economic Analysis*, 1953, p. 261.

[2] Cf. Koopmans (ed.), *Activity Analysis of Production and Allocation*, p. 79.

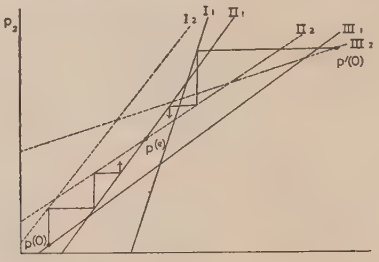

FIG. 2

3. Technological inventions are divided into two categories: inventions of new ways of making old goods, and inventions of new goods (whether final or intermediate). Among the effects of technological inventions we can distinguish long-run and short-run effects. Let us start with the long-run effects of inventions of the first category. Suppose a new activity for making an existing commodity, say the product of the first industry, is invented; then two cases are possible: (1) the invention decreases the minimal equilibrium prices and (2) it has no effect on them. Long-run effects will operate in the first case only. We shall call an invention " effective " if the newly invented activity valued at pre-invention minimal prices is cheaper than any of the old activities. We can prove that the new minimal equilibrium prices (after an invention) will be less than the old minimal prices if the invention is effective (Theorem 5). We can also prove that the newly invented activity is necessarily the activity of industry 1 proper to the new minimal equilibrium prices, provided the invention is effective (Theorem 6). Therefore it is the new efficient activity of industry 1.

If the new efficient activity of an industry changes the efficient activities of other industries, we shall speak of a " trigger effect " being present (see Simon[2]). This is, of course, not necessarily so. Let us assume that the newly invented activity is effective. Consider a Leontief system in which industry 1 adopts the new activity and all the other industries adopt the old efficient activities. A set of equilibrium prices is then determined. We can prove that the invention will have no

[1] *Ibid.*, pp. 142–173.

[2] *Ibid.*, p. 268.

trigger effect on the other industries if and only if at that set of prices the new activity of industry 1 and the old efficient activities of the other industries are the cheapest possible. Conversely it can be proved that if, as a result of the new invention in industry 1, a number of the remaining industries find that activities other than those previously "efficient" have come to be the cheapest (at that set of prices), then the invention must have a trigger effect on at least some of the industries so affected (Theorem 7).

If the long-run effects exist, there must also be short-run effects appearing on the way to long-run equilibrium, as illustrated in Figure 3. Suppose the initial position of prices $p(0)$ is $p^{(e)}$, the minimal equilibrium prices before an invention. Let activity IV_1 of industry 1 be invented in period 0. Before the invention the first industry had adopted activity II_1 and the second III_2; but in period 0 when activity IV_1 is invented the first industry adopts IV_1 while the second sticks to III_2. Thereupon IV_1 and II_2 are selected in periods 1 and 2, IV_1 and I_2 in periods 3, 4 and all subsequent periods. It is clear that the invention has a trigger effect on the second industry, but its full effect is not exhausted in one period. The first industry adopts the newly-invented activity without delay, but the second industry adopts activities III_2 and II_2 before it finally settles on I_2 in the third period.

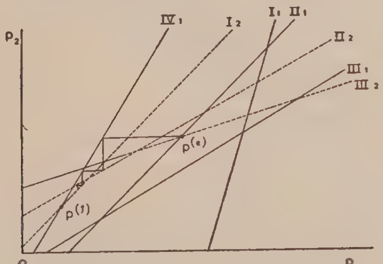

FIG. 3

It may well be that empirical tables of input coefficients (say the 1939 table for the United States) do not represent the kind of long-run equilibrium corresponding to activities IV_1 and I_2 (Figure 3), but merely transitional situations corresponding to periods such as those in which activities IV_1 and II_2 are adopted. Even if there is no new invention in the period represented, the table might still be in process of change as a result of an invention in the past. Thus some of the changes undergone by the 1939 input coefficients in later years may be interpreted as after-effects of old inventions in, say, 1919, though others may be due to 1939 inventions.

Given a Leontief table of coefficients, any set of final demands will determine a unique set of equilibrium outputs. Let us assume, as before, that a new effective activity is invented. Then the system of coefficients which was previously the efficient one is no longer so in view of the wider choice of activities now available, and a different system will become efficient. We can prove that in order to produce the same final demands for goods as before, the efficient system after the invention requires less labour than the one before (Theorem 8). That is to say, the efficient system after the invention is superior to the one before it.

In the above we are concerned with the case in which a newly-invented activity is effective. But if it is not effective, the new minimal equilibrium prices are the same as the old; and hence the invention has no long run effect. But if the system was not in long-run equilibrium before the invention, there may be short-run effects even in such a case, producing transitory structural changes which, however, will disappear in the long run. This is illustrated in Figure 4, where the dotted staircase stands for the time path of prices in the case of no invention, and the solid staircase for the path after an invention has been made.

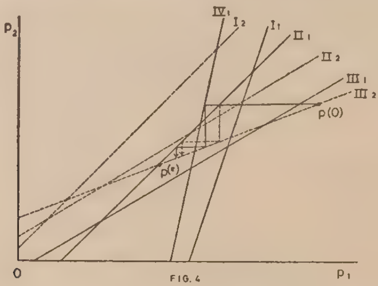

FIG. 4

4. Let us now turn to the invention of new goods.[1] Suppose a number of activities producing a new consumer good (say good $n + 1$), are invented in period 0. After that time, a new industry (industry $n + 1$) comes into existence, so that the Leontief system is augmented to size $(n + 1) \times (n + 1)$. But, since product $n + 1$ is a consumption good, all the elements of row $n + 1$ in the new Leontief table vanish; accordingly, the new $(n + 1) \times (n + 1)$ table is reduced to an $n \times n$ table. It is easily shown that if a new industry is a consumer good industry, its appearance has no effect on the time path of the prices of existing goods and hence on their minimal equilibrium prices. We can also show that the invention has no effect on the input coefficients of the existing industries; that is to say, the structure of the existing industries remains unchanged in spite of the appearance of a new industry.

5. Next we assume that a newly invented good is an intermediate good as well as a final good; then activities of two different kinds will be invented: first, activities to *produce* the new good; and, second, activities to *use* the new good.

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[1] As stated by Coale and Brozen (Koopmans (ed.) *op. cit.*, pp. 277–281), the effects of inventions of new products and new materials are not amenable to treatment by the Simon technique.

Due to the appearance of the new industry the Leontief table is augmented to size $(n+1) \times (n+1)$, but in this case, it cannot always be reduced to an $n \times n$ table. We shall call an invention using the new good in the production of an old one an " effective " invention if the newly invented activity is cheaper than any of the old activities at the old minimal equilibrium prices. We can show that the new minimal equilibrium prices of the existing goods in the expanded system which now includes the new good are less than the old minimal prices if and only if there exists at least one effective invention using the new good in the production of an old one (Theorem 9). It can also be proved that an invention of a new good has no trigger effect on existing industries unless it is accompanied by an effective invention to use this good in the production of an existing good. Furthermore it can be shown that an invention of a new good must have a trigger effect on at least one of those industries which have responded to the new good by inventing new effective activities (Theorem 10).

Suppose there is no trigger effect. Then all the existing industries will adopt old activities which do not use the new good. Since activities are defined as net activities, the new industry itself cannot use the new good. Thus none of the industries in the new situation make use of the new good (say good $n+1$); all the elements of the row $n+1$ of the new efficient Leontief table vanish, so that it can be reduced to the old efficient table which is $n \times n$; but if the appearance of a new industry does have trigger effects, some industries will use the new good, and the new efficient table cannot be reduced to an $n \times n$ table, except in a few special cases, such as the following. (1) If the new efficient activities of industries n and $n+1$ have an identical cost structure, these two industries can be consolidated into one industry.[1] (2) If the new good $n+1$ is a " complete substitute " for an old one, say, good n, in the sense that the newly invented activities in the other industries use it as a substitute for that good (without the latter entering into its production), it can easily be shown that in spite of the appearance of the new good, the table of coefficients will again reduce to an $n \times n$ table once the trigger effects on industries $1, 2, \ldots, n-1$ have worked themselves out.[2]

Thus, if a new industry has the same cost structure as an old one, the latter can be merged with the former; and if a new industry is a complete substitute for an old one, the former drives out the latter. In these cases, the table of input coefficients is not enlarged in spite of the appearance of a new industry. In almost all other cases, however, the table must be enlarged when a new industry comes into existence.

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[1] See Hatanaka, M., " Note on Consolidation Within a Leontief System ", *Econometrica*, Vol. 20, No. 2, April, 1952. The new efficient table whose size is $(n+1) \times (n+1)$ is a " net " table, that is, it has only zeros in the diagonal, but, as McManus points out, the aggregated $n \times n$ table loses this property. See McManus, M., " On Hatanaka's Note on Consolidation ", *Econometrica*, Vol. 24, No. 4, October, 1956.

[2] Because all the elements of the row n of the new efficient Leontief table vanish in this case.

The London Assurance Company and the Marine Insurance Market of the Eighteenth Century*

By A. H. JOHN

The growth of marine insurance facilities represents one of the major developments in the history of English commerce during the eighteenth century. Changes in the law and practice of this branch of business led to the publication of many important contemporary works, and in recent years other aspects have been treated in a number of valuable studies.[1] The aim of this paper is the limited one of supplementing the information already available by an examination of the existing records of the London Assurance Company, one of the two great insurance houses established in 1720.

I

Although the origins of marine insurance in this country go back to the later middle ages, only a small amount of underwriting seems to have been done here before the Civil War—largely because of the competition of more important European commercial centres, such as Antwerp, Hamburg and Amsterdam.[2] The growth of the London insurance market is associated with the marked expansion of English foreign trade during the second half of the seventeenth century; and forms part of that remarkable period of financial activity culminating in the South Sea Bubble, which gave rise to the Bank of England, the Funded Debt and an embryonic Stock Exchange. Marine insurance, it was said, " was indeed here a little before the Revolution, and the breaking out of the late war "[3]: a view which is borne out by the use in London, at least by 1675 and possibly earlier, of three types of printed policies.[4] In its early stages, as might be expected, the market was weak and unsure: " in the late war losses by insolvencies by insurances were very great and the whole sum insured was hardly paid in any considerable loss ". Nevertheless, in addition to an accepted form of contract and a clear understanding of the nature of various risks, a rudimentary organization had already been created by 1700 in the form of a small group of men who made insurance broking their main activity. These brokers, or office-keepers, represented a nucleus of specialists who gave an element of cohesion to an otherwise amorphous market. As intermediaries they received premiums, made payments for losses, and collected " lines " from merchants and others with whom they were in contact.[1] By 1720, when the two marine insurance houses —the London Assurance and the Royal Exchange—were founded, the period of apprenticeship was over. It could then be said, although perhaps not disinterestedly, that " our insurances here, were at present better than ever known to be, and were esteemed by foreigners,"[2] among whom could be numbered the Danish East India Company. Thereafter, the growth of the London market was continuous, although not without its periodic strains, as in 1742–7, 1780 and 1795. By the middle of the eighteenth century London had become the most important marine insurance centre of western Europe. " It is notorious to all the Mercantile World that as the English Insurers pay more readily and generously than any others, most Insurances are done in England."[3] It is virtually impossible to form an accurate estimate of the amount of business then undertaken in London. One guess puts the insurable risks in English foreign trade in 1720 at £20.3 millions, of which some £2.3 millions were underwritten by the two companies.[4] This merely confirms, however, what has long been known—namely, that a large part of coastal and foreign-bound shipping proceeded without cover. It is not until the beginning of the 19th century that the first reasonably accurate account of the volume of business is available. J. J. Angerstein, chairman of Lloyds, in giving evidence before a Select Committee in 1810 stated that the private insurers had underwritten risks to the value of £140,000,000 in the previous year, and the two companies an additional £6,000,000. With an average premium of 7½ per cent. the total premium income was thus upward of £10 millions[5]—almost at its wartime peak, and from which there was to be a sharp fall with the return of peace. By far the greater part of this business was transacted in London.

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* I am indebted to the Governor and Directors of the London Assurance Company for access to their records.

[1] Among contemporary works, N. Magens, *An Essay on Insurances* (1755); J. Weskett, *A Complete Digest of the Theory, Laws and Practice of Insurance* (1781); and J. A. Park, *The System of the Law of Marine Insurance* (1787) are the most important. Recent works include: C. Wright and C. E. Fayle, *A History of Lloyds* (1928); L. S. Sutherland, *A London Merchant, 1695–1774* (1933); and R. Pares, *War and Trade in the West Indies 1739–1763* (1936).

[2] Violet Barbour, " Marine Risk and Insurance in the 17th Century," *Journal of Economic and Business History*, Vol. I. 1928–9.

[3] P.R.O., *Journal of the Commissioners for Trade and Plantations*, 1714–18, p. 340.

[4] One for insurances on ships only ; a second for merchandize only; and a hird for ships and merchandize.

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[1] An interesting account of the procedure adopted in insuring is given in *Comes Commercii* by Edward Hatton, first published in 1712, and which reached its 8th edition in 1747. " When you are minded to insure go to an office in the Royal Exchange and tell those that you find there what you would insure, and in what ship, and desire to know their premiums . . . Your next thing is to satisfy yourself of the solvency of those who are to ensure; and in order to do that, you may desire of the office keeper to give you the names of six or eight of their best men. Which done you may enquire after their credit on the Exchange, or near the places of their abode. Which being satisfied you go to the office and tell them what men you would have and order the policy to be made accordingly to the nature and circumstance of your business ", p. 308.

[2] P.R.O., *Journal of the Commissioners for Trade and Plantations*, 1714–18, p. 340.

[3] N. Magens, *An Essay on Insurances*, Vol. I, p. 430.

[4] The Goldsmiths' Library. Jevons Collection of pamphlets, *Letter to Jasper Vaux*.

[5] B.P.P., 1810, IV. S.C. on the means of effecting Marine Insurances in Great Britain. Evidence of J. J. Angerstein.

Developments in London were paralleled by the growth of marine insurance wherever there were groups of merchants engaged in shipping, not merely in the old-established ports of Europe but also in the growing trans-oceanic centres of trade. There is evidence, for example, of its existence at Fort St. George in India in 1700:[1] and it established itself in many of the thriving ports of the North American colonies in the course of the century. The English out-ports proved no exception to this generalization. Of Bristol, it was stated in 1718, " divers office keepers of this city have for many years past made it their business to procure persons of good substance to insure and to underwrite policies; "[2] and here, too, there was a printed policy in general use by 1746. Exeter, Liverpool, Glasgow, Edinburgh, Newcastle and Hull were other growing provincial centres. Mr. Bartlett Gurney, wool factor and banker at Norwich, for example, " did a considerable business in underwriting at Yarmouth, Lynn and London; but the destruction of the West India Fleet (in 1780) appears to have brought into his mind the risky nature of the enterprise."[3] This multiplication of English insurance markets enabled risks to be shared between them. Thus, *La Nympha*, a prize valued at £150,000, was underwritten at London, Exeter and Bristol in 1747 to the extent of £105,800.[4] It was common for ships to be insured at one port and their cargoes at another: and despite an act of 1746 forbidding re-insurances, risks covered at provincial ports continued to be re-insured in London until late in the century. By 1810 the pattern was further complicated by the growth of particular insurance associations. In the London coal trade, for example, there were two such organizations in the metropolis and twenty in the northern ports, with a total capital of £1,000,000; while there was a Friendly Insurance Society which handled losses and averages—other than capture—on ships hired as military transports. The rates quoted in the provincial markets, as might be expected, were greatly influenced by those ruling in the capital. " There is nothing done here in the way of insurance outward or will," it was written from Glasgow in 1778, " till the premns. are in someway fixed at London by which our people will be much governed ".[5]

Wherever marine insurance developed its origins were always similar in character. They represented a co-operative effort to reduce losses involved in sea transport and were, as a result, " an accidental and not the sole business of merchants ". As such, underwriting remained throughout the century almost entirely in the hands of the merchants. Even the monopoly of joint stock organization granted to the two companies in 1720 contributed to this end, in that the private underwriter was protected from further companies until the second quarter of the 19th century. The concentration of trade in London was also a factor of importance, underlying not merely the growth of insurance facilities but of all the specialized financial institutions. Thus, although the man who was primarily an underwriter had begun to make his appearance by the seventeen-eighties, he was still exceptional in 1800. " It was not until the middle of the 19th century that the predominant position of the underwriter was formally recognised in the constitution of Lloyd's." Nevertheless, the period witnessed further important steps in the institutional development of the market. The tendency to concentrate marine insurance in certain coffee houses fostered a corporate spirit and facilitated the provision of information on the movement and seaworthiness of ships, so essential to the efficient transaction of business. The establishment of Lloyd's List in 1734 by the master of the coffee house of that name, and the founding of Lloyd's Register by a private group of merchant-underwriters in 1764–66, are both expressions of this. Further, the growing volume of transactions inevitably forced private underwriters to meet from time to time, as in 1749 and 1769, to consider innovations in the policy of insurance. The need for a centre of business by the more responsible elements led to the founding of a loosely organized society of merchant-underwriters in 1774 at the New Lloyd's in rooms at the Royal Exchange, where life membership was available on the payment of a £15 subscription. As a corporate body present-day Lloyd's finds its origins in this group. But while the New Lloyd's increasingly became the centre of marine insurance, those who administered the establishment exercised little control over the market generally. Much business continued to be done in the Jerusalem and Jamaica coffee houses, in private counting houses and at the Coal Exchange. Practice, rather than formal organization, remained the most important unifying force, and here, during the 18th century, there was a dual development. There was first a crystallizing of merchant custom and, secondly, its reception into the body of English law, particularly under Lord Justice Mansfield. The importance of this pragmatical method of giving legal sanction to merchant custom cannot be over-emphasized, and it represents one of the subtler ways in which contemporary social institutions influenced economic development. In this process of giving precision to the working of the market, the insurance brokers—of whom there were nearly forty in 1742[1]—played an important rôle. As intermediaries between those who desired cover and those who provided it, they occupied a central position, and by the end of the century can be regarded as having reached the height of their influence. It was at this time that the brokers took to guaranteeing the solvency of their underwriters—a move not everywhere regarded with favour, in that the broker became a party to the policy of insurance. With the rise of the great specialist underwriter, in the

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[1] P.R.O., C/114/23.

[2] P.R.O., *Journal of the Commissioners for Trade and Plantations*, 1714–18, p. 340.

[3] W. H. Bidwell, *Annals of an East Anglian Bank*, p. 35.

[4] N. Magens, *An Essay on Insurances*, Vol. I, pp. 220–235.

[5] National Library of Scotland. Alexander Houston & Company MSS. Letter Book H, p. 336. I am indebted to Mr. L. M. Cullen for this reference.

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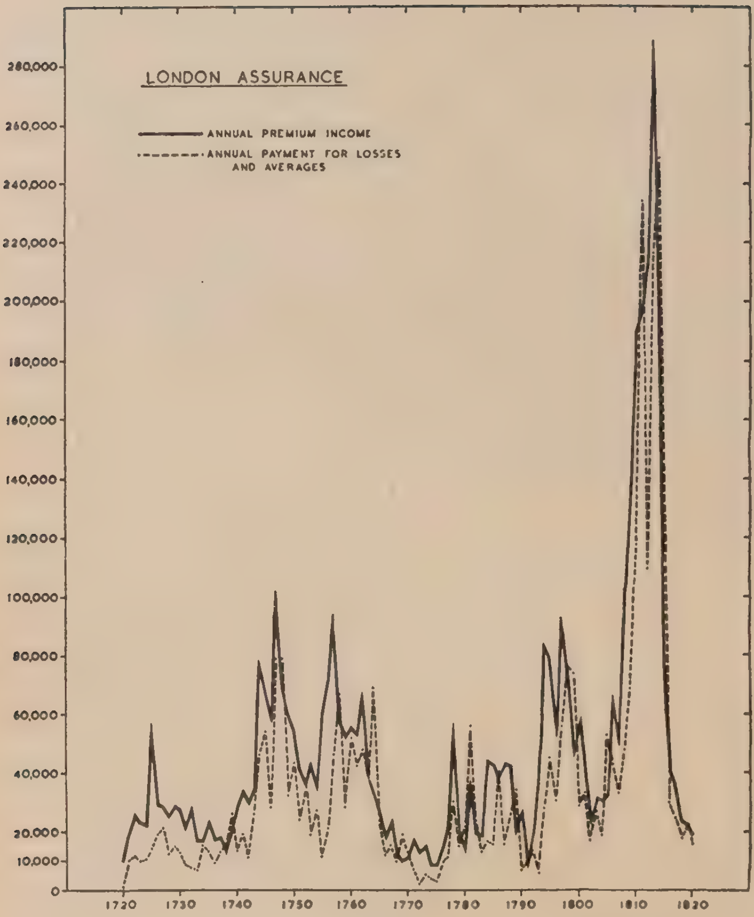
[1] Samuel Glover, *A Short Account of the late Application to Parliament made by the Merchants of London upon the Neglect of their Trade.*


LONDON ASSURANCE
ANNUAL PREMIUM INCOME
ANNUAL PAYMENT FOR LOSSES AND AVERAGES
280,000
260,000
240,000
220,000
200,000
180,000
160,000
140,000
120,000
100,000
80,000
60,000
40,000
20,000
10,000
0
1720
1730
1740
1750
1760
1770
1780
1790
1800
1810
1820

19th century, " assisted by the development of agency and of the syndicate of underwriters ",[1] the need for intermediaries declined, and with it the importance of the insurance broker.

II

It is against this background that the early activities of the London Assurance Company must be examined.

Although its financial structure was that of a joint stock company, its administration, in common with that of similar institutions of the period, reflected many features of partnership organization. In particular, this was shown in the active participation of the directors in the day-to-day business of the corporation, with a consequent absence of any delegated authority to a managerial hierarchy. The work of the company was thus divided up among committees of the Court of Directors. The senior of such committees was that of the Treasury, which was responsible for the finances of the corporation. General questions of administration fell to the Committee of the House. Policies for marine and life insurance were issued by a Committee in Waiting, three members of which met daily and transacted business during the hours at which the office was open.[2] The payment of losses and averages was the work of the important Committee of Adjustment, at whose weekly meetings there were present eight or more directors. Loans on bottomry—never an active branch of business—and fire policies, came within the purview of committees of their respective names. Members of all these bodies were appointed annually by the Court, and substantial fees were paid for the work thus entailed.

The duties of the Committee in Waiting soon involved the issuing of policies to an annual value of about a million sterling; while the sister corporation, the Royal Exchange Assurance, underwrote to the extent of £1,574,505, in the first year of its existence, and continued to do a somewhat larger business. The Committee's records are, unfortunately, very fragmentary, and it is impossible to trace accurately changes in the volume of business throughout the century. But it seems probable that the maximum amount in any one year did not exceed one-and-a-quarter million pounds until 1809–10, when the figure was roughly doubled. The years 1730–39 and 1763–76 were clearly associated with a fall in marine insurance activity, which there is reason to believe was not confined to the London Assurance. It is significant that these periods were characterized by modifications of the insurance policy—some temporary, some permanent—in favour of those who wished to insure. It is possible, too, that during the early wars of the century the two companies might have increased their business temporarily because many considered them to be more secure at such times than the private underwriters. Further, the continuance of a high premium income between 1748 and 1756 suggests that the merchants, at least, discounted the prospect of a permanent peace with France after the Treaty of Aix-la-Chapelle. The immense rise in premium income during the last phases of the Napoleonic War, when England fought the U.S.A. as well as France, is remarkable. What is evident is that, although their individual responsibilities were substantial, the two companies never captured a large share of the

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[1] See, L. S. Sutherland, *A London Merchant*, chap. iii, on the importance of the insurance broker.

[2] Although already influenced by the week-end habit. " Our G.M. busy in the counting house on Saturday morning staid late before going to the London Assurance Office (where we always choose to make Insurances, as thinking it safer than at a private office), and when he came there the Directors were gone, an hour sooner than usual, to take pleasure in the country." Lascelles & Maxwell, Letter Book, p. 38. I am indebted to Dr. R. Sheridan for this quotation.

TABLE 1

GEOGRAPHICAL DISTRIBUTION OF RISKS

A. Direct Risks	1728–29* £	1730–31† £	1769–70‡ £
Coastwise England	44,510	27,193	65,642
Baltic	11,980	16,320	33,054
North Europe	66,085	69,486	143,988
Spain and Portugal	134,632	76,337	263,545
Mediterranean	187,910	120,715	135,589
East Indies and China	66,020	82,750	36,127
North America and West Indies	203,514	221,637	131,996
Africa and West Indies	17,250	11,732	39,688
England and Ireland	6,015	4,650	18,565
Miscellaneous	14,575	—	15,769
Total	752,491	630,820	883,963
B. Cross Risks (not touching England)			
Coastwise Ireland	1,025	100	—
Ireland and Europe and Mediterranean	77,032	26,207	14,122
Ireland—North America and West Indies	3,353	3,755	3,644
Coastwise Europe and Mediterranean	117,592	104,098	85,686
Between West Indies and North America	6,115	23,505	705
Newfoundland—South Europe	13,650	13,275	4,640
Spanish and Portuguese Colonial Trade	58,850	64,150	42,360
French Colonial Trade	4,750	5,600	—
Miscellaneous	13,545	25,001	31,405
Total	295,912	265,691	182,562
Grand Total	1,048,403	896,511	1,066,525

* 1 April, 1728 to 31 March 1729.
† 19 August, 1730 to 27 July, 1731.
‡ 16 November, 1768 to 4 September, 1770 (Two years approximately).

I am indebted to Mr. R. Battersby of the Economics Research Division, L.S.E., for the compilation of these tables.

market. The average premium income earned by one private underwriter—William Braund—during the period 1758–63 came to £8,093 approximately *per annum*, as compared with £54,055 for the London Assurance. And there is no reason to suppose that Braund's business was unusual in its extent.[1] This failure to develop more vigorously is not difficult to explain. Discreetly practised, underwriting was a " gaining trade ", and it was to the mutual advantage of merchants to insure among themselves at Lloyd's or at some other coffee house. The private underwriters were also better able, and thus more willing, to accept risks of all kinds, simply because they could be spread over a large number of persons. The companies, on the other hand, tended to become more conservative in their business as the century wore on. Most important of all, perhaps, was the fact that the London Assurance at least—and possibly the Royal Exchange Assurance—required brokers, as it did its directors, to make a deposit as security for their accounts. The company thus challenged the dominant position of brokers, and business, as a result, tended to be directed elsewhere.

The geographical spread of risks assumed by the company reflects the pattern of contemporary English commerce. Most of the major trade routes are represented and there are also a number of time risks. There is a judicious mixture of direct and cross risks and a predominance of insurances on outward over inward voyages. Only a few policies deal with both out and home journeys—for the most part in the trades to the East and West Indies and to North America. Among such policies in 1728 are a number on the round voyage from London to Madagascar, Buenos Aires, West Indies and London: and in 1770 on ships to and from the Greenland fisheries. The two largest items among the cross risks refer to the insurance of ships and goods in Europe and in the colonial trade of Spain and Portugal. Into the first of these categories fall the policies dealing with trade in and out of Rotterdam, Amsterdam and Hamburg; but of greater significance is the large number of insurances placed in the French coasting trade, in the commerce between France and Spain, and between France and Ireland. Other risks of this kind are represented by the movement of grain from Sicily to Spain; by cargoes from Genoa to St. Petersburg; and by some in the French colonial trade. From the 'thirties onwards the company grew chary of cross risks in distant waters, alleging that " they had suffered a great deal on vessels in a remote part of the world, where it was pretended sometimes they had sustained damage by bad weather, when they believed they were only sent out ill-fitted, and by that means they had paid in making almost new vessels from very bad ones and old ones, and therefore they had now made a standing rule not to insure to pay any partial loss ".[2]

But while the business of the company reflected the general range of risks available in London during these years, the marked importance of insurances in the south European routes, in the route to the West Indies, and in the French trades, was the result of its position *vis-à-vis* the insurance brokers. For a substantial—if not the greater—part of the business brought to the company's office was effected for, or through the agency of, its directors and shareholders. Many of these were merchants who placed insurances for themselves and their correspondents,[1] and some were well known insurance brokers. As a consequence, the risks underwritten mirrored the interests of the predominant groups within the corporation. Of particular importance was the Huguenot element with its trading connections in Europe. "To illustrate this point it is not necessary to go beyond the directorate of the London Assurance during the first half century of its existence. Here we find Artouin, Aubert, Aufrere, Bosanquet, Bréholt, Crespin, Fauquier, Gaultier, Jamineau, Langlois, La Porte, Laprimaudaye, Loubier, Minet, Molinier and Pigou."[2] Closely allied to these, as far as their regional trading interests are concerned, were the representatives of the Irish Catholic merchants in France, Portugal and Spain, of whom George Fitzgerald and Thomas Thomas were among the most important; while the Vernons, the Radcliffes, David Martin and Godhard Hagan were prominent members of the trading interest to the Levant and the Iberian peninsular. John Hyde and J. D. Bréholt, supported by sugar factors such as Lascelles and Maxwell and J. A. and J. T. Rucker, brought insurance in the West India trade. The presence on the board in the last quarter of the century of Abraham Mello, of John Dorrien, Mello & Company, explains in part the growth of business to and from Hamburg. All this was but an expression of the significance of family, religious and national groupings in English industrial and commercial life during the 18th and much of the 19th century.

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[1] L. S. Sutherland, *A London Merchant*, p. 69.

[2] London Assurance. Minutes of the Committee of Adjustment, December 1732.

The same factor, to a large extent, explains the noteworthy paucity of coastal and north European risks, despite the substantial traffic in these waters. It is also true, of course, that many of those engaged in the coastal trade insured locally, and many more were their own insurers: and that Amsterdam was still a powerful rival. Of the coastal policies in the surviving minutes books, a substantial number relate to voyages beginning or ending in London; many of which deal with the carriage of metals. Thus in the 'twenties, Jacob Wachter—another director—insured the carriage of lead ores along the Welsh coast to Chester, and lead from Chester to London; while fifty years later consignments of copper from Neath and Chepstow, and lead from Newcastle, required insurances running into many thousands of pounds. Most of the business done in north Europe was in the London-Hamburg trade, and direct risks between England and France were exceptional. The failure of the French harvest in 1725, for example, resulted in a number of policies on corn shipped from various ports from Yarmouth to Tenby. In the early years of the War of the Austrian Succession, Spanish goods were brought in French ships (then neutral) to the northern ports of France and there transhipped into English vessels: from Santander, for example, to Dunkirk; and from Dunkirk to London. Similarly, Godhard Hagan insured indigo to the value of £10,230 from Nantes to Painbeuf for trans-shipment to London in 1770.

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[1] The complexity of contemporary insurance business and the difficulty of deciding for whom policies were taken out is well illustrated by the following memorandum made on 9 May, 1728:—" Mr. Solomon De Medina, Moses, Son and Compa. having made ye 29th July, 1724 £2,000 Assurance on goods from Buenos Ayres to Cadiz upon Interest brought or ship'd by Don Pedro Prudencio or his Execrs and consigned to Sir William Terry for account of Joseph de Medina and Sons of Amsterdam on any ships then next expected from Buenos Ayres to Cadiz, vizt the St. Raphael, the St. Carlos and St. John Baptista at £4.4 per cent. The said Solomon de Medina, Moses, Son and Compa having endorsed the policy over to Francis Salvador jnr. and the said Francis Salvador producing two certificates upon oath dated 5 January last one signed by Sir William Terry of Cadiz and the other by Don Josh. Fernandes Romero, Commander of the St. Raphael and Don Thomas De Luberriaga, Commander of the St. Carlos and which mentions that Don Pedro Prudencio had shipped no goods as aforesaid, the Committee agree to return the said Francis Salvador jnr. £74 half per cent. being deducted as usual, but at the Request of the said Francis Salvador jnr. the Committee order the said £74 to be carried to the credit of the account of Danl de Flores."

[2] Bernard Drew, *The London Assurance*, p. 217.

Insurance on foreign account is of exceptional interest, representing as it does the beginnings of a major item in British invisible exports. An entry on a defaulting borrower in the minutes of the Committee on Bottomry sheds light on another item of these exports, namely the foreign use of British shipping. In the eighteen months after October, 1726, the movements of Captain Hull, in the *Elizabeth*, were:

> " from London to Civita Vecchia, thence to Naples, thence to the Gulph of Venice, and from thence to Lisbon, from whence in August 1727 he took his lading for Marseilles, and from thence proceeded to the Gulph of Venice, from whence he returned to Lisbon."

It is difficult to estimate the precise proportion of the Company's premium income received from foreign sources, but it must have been, like that of the insurance market generally, substantial. Among the direct risks, sugar and tobacco consigned for sale in London by West Indian and American planters was a large item of such earnings: as also was a major part of the cross risks undertaken by the company. The reasons for the growth of this aspect of London's marine insurance were many and complex. There was the decline of Dutch merchant activity and of Amsterdam as the ' mart of Europe ' at a time when international trade was expanding rapidly, and becoming increasingly focused on trans-Atlantic markets. English and French merchants stepped into the places vacated by the Dutch with a consequent increase in the auxiliary activities of commerce. In this contest of succession, however, France suffered a number of handicaps. Although the value of her trade quadrupled between 1720 and 1780 it was never as balanced in character as that of England. The specialization of French ports in particular branches of trade was also a disadvantage. For there was no single centre, such as London, where there was a concentration of mercantile wealth based on a diversity of trades: and hence no single market which could undertake with safety the volume of insurance business transacted in the English capital. France suffered too from an absence of sound financial institutions—not least a well managed national debt: while " the fighting ability of the British warships, in days when Barbary pirates were still active, offered a remarkably cheap form of insurance of which French and Dutch merchants and even the Spanish government regularly took advantage ".[1]

Included among the foreign risks were those on enemy property during the numerous wars of the century; and in 1746 alone the London Assurance paid £18,000 to Spanish and French merchants for losses largely occasioned by British ships. In this, the company followed the general practice of the market, which freely undertook this business, especially during the War of the Austrian Succession when a collapse of French underwriters took place in 1744. The practice was forbidden in 1748, but the Act lapsed with the end of the war; and although there were discussions on the subject at the beginning of the succeeding conflicts there was no similar legislation until 1793. Further, an enactment of 1752 which forbade insurances on foreign vessels trading to the East Indies was repealed in 1758. The insuring of enemy property was thus not only legally permitted but openly practised until the beginning of the Revolutionary Wars. " When Rodney, in 1781, took advantage of the Dutch declaration of war to seize the island (St. Eustatius), with accumulated stores valued at £3,000,000, a howl of execration arose from the merchants, and there is too much reason to fear that many underwriters at Lloyd's may have had heavy claims to settle."[2] The courts, under the influence of Chief Justice Mansfield, who as Solicitor-General had opposed the Act of 1748, took a liberal view of this activity based entirely on expediency:

> " It is for the benefit of this country to permit these contracts upon two accounts; the one because you hold the box, and are sure of getting the premiums at least, as certain profits; the other because it is a certain method of gaining intelligence of the enemy's designs and I have known instances of intelligence procured by such methods."[3]

The weakness of this position—apart from its morality—lay in its conflict with two well known principles of the Common Law; namely, that trading with the enemy was illegal and that the enemy had no *locus standi* in the English courts. For this reason, as well as because of the temporary Act of 1793, his successors came to regard marine insurance contracts with the enemy as void.

During the first twenty years of the company's existence, while the size of individual risks varied greatly, most were under £2,000 and more than half under £1,000. Larger risks were, on the whole, unusual; and were mainly to be found in connection with the small but rich argosies sailing from the Levant, in the Spanish and Portuguese colonial commerce, and in the English East India and Baltic trades. In all of these the cargoes—especially those homewards—were of considerable value, the ships employed were relatively large, and the majority of the traders were among the richer merchants. In July and August, 1725, for example, policies on three ships from Scanderoon and Cyprus to London amounted to £12,100, £15,600 and £9,350 respectively; and eleven years later those on the *Delawar*, on the same voyage, came to £22,600. These were large single risks to undertake when the average " line " among private underwriters was £50–£100 in 1700 and about double that sum fifty years later; and when £12,000 and £22,000 were quoted as large losses paid by office keepers.[1] By 1770 the relatively big risk occurs more frequently, particularly in respect of textile raw materials such as cochineal, indigo and wool. The change in the average value of policies to and from the Iberian Peninsular is very striking, being under £500 for 1730 and over £1,000 in 1770. These facts might well indicate a growth in the number of larger merchant houses, especially in London. By the end of the century, the company was reported to have grown more cautious in the matter of large individual risks, and to have imposed a maximum of £10,000; which was regarded as another factor in driving business to the private insurer.

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[1] A. Christelow, " Great Britain and the trades from Cadiz and Lisbon to Spanish America and Brazil," *Hispanic-American Historical Review*, 1947, p. 20.

[2] Wright and Fayle, *op. cit.*, p. 157.

[3] Quoted in J. A. Park, *op. cit.* (1st Edition, 1787), pp. 376–7.

Granted the existence of peace or war, the premiums on various voyages do not seem to have changed much between 1720 and 1770. There is, indeed, no reason why they should have changed. The size of ships altered little in the course of the century, and the hazards of individual voyages were well understood by 1700. In peacetime the most important difference lay in the summer and winter premiums for voyages in certain waters. It occurs infrequently in the routes to southern Europe and the American colonies, but was common to some islands in the West Indies, especially Jamaica, the English coasting trade, and the North Sea. The premiums to Hamburg in 1769–70, for example, were April to September 20s. per cent.; November to January 50s. per cent.; falling again to 21s. per cent. in March. The sailing hazards of the Caribbean produced a relatively complicated structure of rates for the West Indian islands, but it is noticeable that, at all times of the year, the premiums from Jamaica were twice those charged on the outward journey.[2] With the onset of war conditions, premiums rose sharply, especially for ships proceeding through the English channel. This can be well illustrated from the records of the Committee in Waiting for 1739–40. Rates between Genoa and London were: June 21s., July £3, August 5 guineas (2 guineas abated if with convoy),

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[1] Samuel Glover, *A short Account of the late application to Parliament made by the Merchants of London upon the Neglect of their Trade.*

[2] For an account of these hazards see R. Pares, *War and Trade in the West Indies*, especially chapters 9 and 10.

October 1739 to March 1740, 8 guineas per cent. (3 guineas abated if with convoy). Rates across the Atlantic were higher, and on enemy insurances higher still. Voyages from Lima to Cadiz, or Martinique to Nantes, in 1744 cost £36¾ per cent.; and Bordeaux to Martinique £25 per cent.[1]

TABLE 2

PREMIUMS PER £100 TO AND FROM LONDON

		1730–1*		1768–70	
		S	W	S	W
		Shillings		Shillings	
Hamburg	out	21	40	20	—
	home ..	—	40	20	40–42
Amsterdam	out ..	—	25–30	20	—
	home ..	—	—	—	—
Bilbao	out ..	30	30	25	30
	home ..	25	25	25	25
Cadiz	out ..	—	—	21	25
	home ..	21	25	21	25
Leghorn	out ..	30	30	30	30
	home ..	30	30	30	30
Scanderoon	out ..	30	40	35	40
(Smyrna)	home ..	30	—	30–35	40
Dublin	out ..	—	—	—	25
	home ..	—	30	21	30
North America	out ..	40	40	42	42
	home ..	40	40	42	42
Jamaica	out ..	50	50	50	50
	home ..	80	100	84	84
East Indies out and home ..		6 guineas		15 guineas (£8 returned if no hostilities in European waters)	
East Indies	out ..	—	60	—	—
	home ..	—	—	84	—
Newcastle-on-Tyne	out ..	—	—	—	—
	home ..	—	30	15	20

S—Summer.
W—Winter.
* In all cases to return 84 per cent. in case of loss.

The growing strength of the London insurance market was shown not so much in the reduction of its premiums as in its growing efficiency and in the value of its policies. The basis of the market—the contract of insurance—was in its essentials a venerable document when the two corporations were established in 1720. The use of a printed form by 1675 is evidence of a consensus of agreement on the nature of the risks involved, particularly as its authority "depended entirely on its general and voluntary acceptance by all concerned".[2] As marine business expanded there grew up a mass of interpretation which, while making the policy sacrosanct, also tended to render automatic the adjustment of a widening range of losses. This in turn was a factor in attracting more business. The process of creating precedent is aptly summarized in the minutes of the Committee of Adjustment in 1724: " Mr. Linwood who appeared on the 10th inst., again appearing, was told by the Committee that as to the point of Deviation which was a matter of Law they would refer it to Such Councel Learned in the Law as Should be mutually agreed on. And as to other matters in dispute they were ready to refer them to Merchants Skilled in Trade. And he was desired to consider of the same and give his Answer."[1] In this process Parliament played little part, except to curb the element of gambling and to give legal sanction to the practices of the market, as it did in 1795 when the form of marine policies was confirmed by statute. Earlier in the century, in 1748, a committee of the House of Commons considered a series of ordinances for marine insurance, but the matter was never pressed. Market practice was, however, influenced in a minor way by the needs of Government revenue. By the middle of the century stamp duties discriminated heavily in favour of country policies, being 1s. in the provinces, 3s. 10d. for private insurers within the Bills of Mortality, and 4s. 10d. for the two companies. To avoid these duties, individual policies were contrived to cover a series of cargoes or voyages; and recourse was had to letters and memoranda in place of official contracts. It was not until 1764 that a plan was approved by the Commissioners of the Stamp Office and Mr. Heaton, solicitor of the London Assurance, by which, in future, policies should refer to " one person, ship or insurer ".[2]

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[1] N. Magens, *An Essay on Insurances*, Vol. I, pp. 141 and 147.
[2] C. Wright and C. E. Fayle, *op. cit.*, p. 143.

The basic clauses of the contract of insurance were common to all policies[3] but it was usual to insert temporary and specific conditions for individual voyages. A merchant might effect an insurance " free of average " which in some cases was 20 per cent. cheaper than the normal premium: slaving risks were accepted subject to the clause " the assured doth hereby agree to warrant the ship sheathed, to take upon himself all loss or damage by death or insurrection of slaves and all loss or damage by prohibited trade." The only important and generally accepted modification during this period was one which referred to small or partial losses (averages), codified in the " Memorandum of 1749 ".[4] This dealt with small particular averages, and particular averages on certain types of cargo specially liable to damage by sea or deterioration in transit. It is clear that this addition to the policies was a crystallization of long existing practices, but it is interesting to see how, in some respects, these were abandoned in the 'thirties when marine insurance business declined. Corn, for example, was originally insured 'free of average'; but "the Court being informed that several gentlemen had been obliged to insure their corn ships elsewhere by means of the clause free from average insisted on by the Corporation: Ordered that in future the Committee in Waiting should not insist on corn being free from average".[1] As far as bulk is concerned, grain represented one of England's most important outward cargoes between 1730 and 1749, and as a consequence losses must have been considerable. It was not until the greater demand for insurances during the War of the Austrian Succession that the market generally reverted to its earlier and more rigid practice. Similarly, in the post-Seven Years' War recession in marine business unsuccessful efforts were made to introduce clauses which would have allowed merchants to offset unpaid premiums against losses due to them and admitted the principle that "any Insufficiency of the Ship, or Deviation of the Master, unknown to the Assured" should not affect the policy.

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[1] London Assurance. Minutes of the Committee of Adjustment, January 1723–4.

[2] B.M.Add.MSS. 38339, f.70 *et seq.*

[3] The private underwriters continued to use the traditional three policies. The two companies each had their own policies, which were used for all categories of marine insurance.

[4] The final version of the Memorandum ran:—" Free from all average on corn, fish, salt and seeds, except general. Free from averages on sugar, raw hides, skins, hemp, flax and tobacco under 5 per cent.; and on all other goods under 3 per cent. except general."

It is possible, however, that in one important respect the conditions of business in these years exercised a permanent effect on the value of London policies. Earlier contracts of marine insurance included a clause stipulating an abatement in case of loss. This was generally 10 per cent. but custom dictated another 5 per cent., "which many," wrote Hatton, "think much to have abated".[2] As a consequence, merchants took to insuring the abatement as well as the cargo or ship. By 1720, at least with the London Assurance, two types of insurance were offered: one to repay 84 per cent. in case of loss and the other 98 per cent., which might have been a response to the insuring of the traditional abatement. Thus in 1739 Sir Joseph Eyles insured £2,000 on the *Delawar* for 84 per cent. repayment at a premium of 30s. per cent.: while Edward Radcliffe affected an insurance on the same ship for the same voyage at 35s. per cent. to repay 98 per cent. in case of loss. Most of the policies in the early years of the company appear to have been for the lower sum on repayment. When the practice of returning the full loss was established cannot be precisely stated. But the minute book for 1769–70 shows that all policies were then entered for repayment at 100 per cent. less 10s. per cent. abatement. The change must clearly have taken place some time during the middle of the century, and might possibly be associated with the scramble for business in the years after the Seven Years' War. But whatever the explanation, the effect was to pay 99½ per cent. on losses—other than those covered by the Memorandum of 1749—for a premium which earlier obtained only 84 per cent. There can be no doubt that this innovation was adopted by the market generally, and was one from which there was no subsequent change.

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[1] London Assurance Court Minutes 1734.

[2] Edward Hatton, *Comes Commercii*, p. 308.

III

If the concept of a " take-off " into a " self-sustained economic growth " has any validity, then in the British case the development of a highly efficient set of financial institutions must be numbered among the important pre-conditions of that event. For when industrial expansion gathered pace at the end of the 18th century, English hegemony in world commerce and finance was already in process of being established. The value of the records of the London Assurance Company lies in the way they illustrate a particular aspect of this development, which formed part of the traditional system of entreports in international trade. In so far as contemporary governments, well into the 19th century, pursued a positive economic policy, it was directed to the encouragement of London in this capacity. Out of the success which attended these efforts sprang the invisible exports of this country which were to account for so large a part of English overseas earnings and eventually to contribute in a substantial way to British standards of living. The parallel growth of industry naturally had its effect on this development; but equally the financial strength of London, and the provincial commercial centres, played a fundamental rôle in the development of commodity exports by the provision of extended commercial credits and later by the great volume of long-term capital investment.

The London School of Economics.

The English Capital Market Before 1914

By A. K. Cairncross

In his Note in the February, 1957 issue of *Economica*, Mr. A. R. Hall takes issue with Lavington and " most subsequent writers " on the deficiencies of the English capital market as a source of funds for home investment before 1914. He claims that while Lavington's conclusions may be true of the years 1911–13, they are not true of an earlier period when home investment was higher. As one of the " subsequent writers " I remain unconvinced.

There are, in fact, three propositions in dispute. The first relates to the importance of the new issue market in *total* home investment; the second to its importance in providing funds for new investment in British *industry*; and the third to its *efficiency* as an instrument in providing the capital that industry required.

There is no doubt that the relative importance of home and foreign issues on the London market was quite different in the years immediately before the first world war than in the years before the Boer War. The diminution in home issues was not nearly so great, however, as Mr. Hall suggests. He is content to take as a basis of comparison the amount of capital offered, not the actual amount subscribed, and it happens that in the late nineties there was a big discrepancy between the two. Let us take the years that Mr. Hall suggests as marking the contrast most vividly, 1911–13 and 1896–98. For both periods we may use the data published in the *Investor's Monthly Manual* from which *The Economist* took its annual totals. Mr. Hall has made a rough classification of the figures of new issues for the years 1896–98 and Mr. G. L. Ayres made estimates for the years 1911–13 in his unpublished thesis.[1] Mr. C. K. Hobson, at a much earlier date, analysed the total for actual money calls from 1870 to 1914 and published totals for home and foreign issues in his *The Export of Capital*.[2] Hobson's figures compare as in Table 1 with those of Ayres and Hall.

If we take capital subscribed as a basis we find practically no change in home issues between the two periods selected; and home issues in 1896–98 work out very little higher than foreign issues in spite of the boom in home investment and the slump in foreign lending in those years.

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[1] G. L. Ayres, " Fluctuations in New Capital Issues on the London Capital Market 1899–1913 ". M.Sc.(Econ.) Thesis in Library of University of London.

[2] C. K. Hobson, *The Export of Capital*, pp. 209–19. Hobson was well aware of the imperfections of new issue statistics as a guide to fluctuations in investment as his remarks on pp. 209–11 show.

But this is not all. The issue of capital on the stock exchange was largely for the purpose of disposing of flourishing concerns, hitherto in private hands, to new owners. Issues of this kind added little or nothing directly to the sum total of home investment. In the year 1899, according to Ayres[1]—and I take 1899 because no figures for earlier years are available—79 per cent. of the value of issues by " newly-formed " companies (other than railways, canals and docks, public utilities and financial undertakings) was required for payments in cash or issues to vendors. In 1896–98, Mr. Hall's own tabulation shows that issues by breweries and distilleries averaged over £18m. per annum out of the total of £80m. for all home issues. No one supposes that at a time when the rate of net investment in British industry was probably under £50m. per annum more than a few millions at most can have found their way into breweries and distilleries. A substantial proportion of the capital raised went to the vendors in circumstances where the prospect of a public issue was almost certainly not in the minds of the vendors when they built up the assets of the company. To this extent, new issues were irrelevant to the flow of funds into home investment either currently or in the recent past.

TABLE 1

ENGLISH NEW CAPITAL ISSUES, 1896–98 AND 1911–13 ACTUAL MONEY CALLS AND OFFERINGS FOR PUBLIC SUBSCRIPTION

	1896–98		1911–13	
	Hobson	Ayres & Hall	Hobson	Ayres & Hall
	ANNUAL AVERAGE			
Home Issues ..	39·5	79·8	35·8	31·9
Foreign Issues..	41·7	61·1	127·5	149·0
Total	81·2	140·9	163·3	180·9

The opposite situation can be illustrated from Mr. Hall's figures for railway investment. The capital expenditure of British railways in the years 1896–98 was much higher—on my estimates, two thirds higher—than the average of £8.8m. which they issued during those years. The local authorities were certainly investing more than the £6m. per annum that they were issuing for public subscription; their accounts show receipts from loans of nearly £20m. per annum. There has never been any suggestion that the railways and local authorities made little use of the new issue market; but these figures show how far they were from complete dependence on it for new capital.

If these two points are taken in conjunction with one another—the inflation of new issues by the steady conversion of private to public companies and the exclusion from new issues of large amounts raised elsewhere by some of the principal clients of the new issue market—it becomes almost meaningless to set a series for new issues alongside a series for home investment: still more, when the principal item in home investment is house-building which was rarely, if ever, financed from new issues. But let us make the comparison. Does any contrast then emerge between the two periods shown in Table 1? The answer is plainly " no ". Net home investment, both in 1896–98 and in 1911–13, was probably about £120m. per annum and the sums subscribed through the new issue market averaged £39.5m. in the first and £35.8m. in the second, so that there was no very great difference on either count between the two periods. The amount of money passing through the new issue market represented about one-third of the total of net home investment in the earlier years and this is certainly higher than the 10 per cent. attributed, quite correctly, to me by Mr. Hall. But the figure of 10 per cent. relates explicitly to " new issues on the London Stock Exchange by industrial concerns " and the context makes clear that issues by railways, investment trusts, estate companies, etc. are excluded as well as cash payments to vendors.[1] If we take the year 1899 (instead of, as I did, 1907) the £39.5m. subscribed is far above the amount that flowed through the new issue market into new investment. I have analysed the money calls during the first six months and it is quite apparent that nearly all the larger calls were for railways, trams, and local authorities. Excluding this group, the average for the six months is no more than about £1m. per month, leaving out brewery issues altogether but making no special deduction for payments to vendors. In 1907 the comparable figure, excluding cash payments to vendors, was £13.8m. The difference is insignificant; so that if my 10 per cent. stands for 1907 it stands also for 1899.

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[1] *Op. cit.*, p. 41.

Let us turn to the second issue. Did British industry get any large proportion of its capital through the new issue market? The answer seems to be that the market before 1914 was mainly occupied in converting private into public companies and that this frequently led to large new issues but that money furnished through the stock exchange for investment in new assets was small in amount both absolutely and in relation to total industrial investment. Most of the industrial capital of the country at the end of the century was still in the hands of private companies and family businesses with no access to the new issue market for funds. Where they had already been converted into public companies they relied heavily on reinvested profits rather than on fresh issues. At times of great speculative activity, it was possible to float completely new companies in the trades that were booming; but no large amount of capital was involved and new issues of this type were normally confined to light industries providing new consumer goods and services.

All this has been said many times and it is not clear to me whether Mr. Hall is seeking to controvert it or not. His own figures offer striking support of the accepted view. For example, in 1896, we find the following:

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[1] Cairncross, *Home and Foreign Investment, 1870–1914*, p. 96.

TABLE 2

NEW CAPITAL ISSUES IN 1896

	£m.
Breweries and Distilleries	18·2
Railways, Public Utilities and Local Authorities	16·9
Stores and Trading Companies, Hotels, Theatres, etc.	13·6
Cycles and Appliances	17·2
All other Manufacturing	12·8
Miscellaneous	10·4
	89·1

This shows how speculative excitement enabled more capital to be raised for new cycle companies than for all other manufacturing activities except brewing, where company promoters were busy converting large numbers of well established firms into public companies.[1] How much of the £17.2m. shown in Table 2 was subscribed and in due course invested we do not know; but the magnitude of the total for new issues by cycle companies in comparison with the modest total for " other manufactures " is hardly in keeping with the picture of an efficient capital market that Mr. Hall seeks to present.

If we take 1899, for which Ayres gives a full analysis,[2] we can find ample confirmation of the subsidiary position occupied by the new issue market. The cotton textile industry, employing over 500,000 workers, offered only £250,000 for public subscription in that year; the chemical industry, £588,000; the coal industry made no issues at all. Even the iron and steel, engineering and shipbuilding group made issues totalling only £8.7m.—about the same total as the breweries.

Unfortunately, we cannot pursue the analysis further because we know neither the total for new industrial investment, nor the source of the funds from which this investment was financed. Perhaps we never shall know; but further research might give us at least a more adequate basis for guessing. So far as I can judge, manufacturing industry had a total capital in 1907 of not more than about £1,000m. and net industrial investment cannot have exceeded some £50m. in that year. Making a wild but plausible guess, if two-thirds of this came from profits, and only one-tenth from new issues, other sources of borrowed capital would be left to provide twice as much as the new issue market.

The figures for 1899, moreover, can hardly have differed greatly from the figures, whatever they were, in 1907. It is not possible to assume that because *total* home investment fell off after 1900, *industrial* investment must have followed the same course. On the contrary, the great activity of the export industries after 1900 and the high rate of profit obtainable may well have led to more factory building than before. There is no doubt at all, for example, of the increase in new shipbuilding for British owners or of the increase in iron and steel consumption. My own bogus series for " machinery retained for home use " rises from £23m. in 1896–98 to £32m. in 1911–13. It is one of the constant problems in applied economics that propositions that are true of industrial capital are not true of total capital and *vice versa*.

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[1] H. Osborne O'Hagan, *Leaves from My Life*, ch. xxii.

[2] G. L. Ayres, *op. cit.*, Table 13.

Lastly, was the capital market efficient? Mr. Hall points out that it is possible to get along satisfactorily without issuing houses and company promoters, that there may have been no major function for such intermediaries to perform and that the worst excesses of the London market were to be found among overseas promotions. It is impossible, in a concluding paragraph, to deal effectively with so large an issue. So far as he is trying to combat the view that " the relative inefficiency of British industry in the decade or so before 1914 " was attributable to " a bias in the direction of the flow of funds ", I agree entirely with him; it was not shortage of finance that made British industry slow to make full use of scientific knowledge. So far as he is emphasising " the absence of any urgent need or desire on the part of business men for Stock Exchange facilities ",[1] he is repeating what has already been said by those who have read their Lavington. But he is obviously going a great deal too far if he suggests that better and cheaper facilities could not have been desired for the small British industrialist.

University of Glasgow.

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[1] Cairncross, *op. cit.*, p. 102.

Some Strange Marriages Between the Income-Expenditure Lag and Variable Prices: A Reply

By W. Beckerman

Mr. Spraos's recent article[1] is composed of two sections. The first shows that some logical difficulties arise when one attempts to derive satisfactory demand functions involving real income from expenditure equations used in some international trade models by Mr. Lloyd Wright, Professor Stolper, and by myself in an article published in 1952[2]. On this point I quite agree with Mr. Spraos. I had made the same point myself, though admittedly for different reasons, about four years ago.[3] However, the defects noted by myself and Mr. Spraos do not, I think, affect the main point of my model, which was simply to show the type of modifications that have to be introduced into the stability conditions for foreign trade multipliers when prices as well as incomes are allowed to change. But it is not worth while dwelling on this topic, because since 1952 it has been the subject of several very superior publications which have made my old model quite obsolete.[4]

Much more important, in my opinion, is the second section of Mr. Spraos's article, which is concerned with the empirical validity of a certain assumption concerning consumers' behaviour. This section of his article consists entirely of criticism of my assumption that expenditure on a given product in any period is a function of income in the preceding period and prices *in the preceding period.* Mr. Spraos says, " Surely this is very strange," and goes on to assert that it is a " well established partial postulate " that " given constant money income, the demand for any commodity reacts to a change in its price without a lag ".

Mr. Spraos's only evidence in support of this postulate is the following: " When I go to a second-hand bookshop I do not ask what prices were charged yesterday for the various books I fancy. This is, as far as I am concerned, irrelevant history; what matters in weighing the merits of buying the books as against those of, say, a new shirt, is the prices charged today."

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[1] " Some Strange Marriages between the Income-Expenditure Lag and Variable Prices," *Economica*, May 1957.

[2] " Price Changes and the Stability of the Balance of Trade," *Economica*, November 1952.

[3] " A Note on Variable Prices and Foreign Trade Multipliers," *Review of Economic Studies, 1953-54*, vol. xxi (2), No. 55, p. 163, n. 2.

[4] The very recent article by Mr. Brems (" Devaluation, a Marriage of the Elasticity and the Absorption Approaches," *Economic Journal*, March 1957) contains a fairly full list of references to the relevant literature. Mr. Brems is, incidentally, much more charitable than Mr. Spraos in that he notes with approval the criticisms made by Professor Machlup and myself of the then state of the theory on this subject. In the first paragraph he even goes so far as to give me the credit for a criticism which I never made, and with which I disagree.

Now it is not really clear how far this operation helps Mr. Spraos to allocate his expenditure according to today's *relative* prices. True, he will know the current price today of the books he is buying but he will not know their relative price unless he also enquired into the prices, on the same day, of all the items which might be competitive with books in his consumption pattern. This would take a long time. The relative price of the books that he knows is therefore a rather hybrid price being compounded of today's absolute price of the books and yesterday's absolute prices of most other items.

Thus the lag assumption seems perfectly realistic as an empirical description of how consumers do in fact behave. It may well be that they are trying to allocate their expenditures on the basis of current prices, and in so far as Mr. Spraos's account of his own shopping behaviour is to be interpreted solely as a description of such attempts, it is probably a fair one. But it is not absurd to suggest that these attempts fail. It may also be true, as Mr. Spraos claims in his Rejoinder below, that the reason for such failures must be one (lack of perfect knowledge) which might raise other logical difficulties for my model. But this is quite a different matter.

O.E.E.C., Paris.

A Rejoinder

By John Spraos

I accept *in principle* that, in so far as the consumer is ignorant of some relevant current prices, his allocation of income between individual goods objectively depends on some (not all) which are out of date. But I doubt if this will do as a defence of Mr. Beckerman's initial position, since, as he acknowledges, the " ignorance " hypothesis could be a source of other logical difficulties for his model.

University College, London.

Demand Theory Further Revised *or* Goods Are Goods

By Harry G. Johnson

In his review of Professor Hicks' *A Revision of Demand Theory*, Kelvin Lancaster shows that the fundamental proposition of demand theory, that price and quantity demanded tend to move in opposite directions, can be derived from the idea of consistency.[1] It can, however, be derived even more simply from the hypothesis that goods are goods, as follows:

Define a good as an object or service of which the consumer would choose to have more. Then the collection of goods he chooses when he has more money to spend (prices being constant) must represent more goods than that he chooses when he has less money to spend (since he could have had more of each separate good).

(i) If his income rises, he buys more goods; this implies a presumption that normally the income-effect is positive.[2]

(ii) If he chooses collection B when he could have had collection A for the same money (i.e. $\Sigma\, p_b\, q_b = \Sigma\, p_b\, q_a$), he does not choose A if he could have had B for less money, because that would mean collection B represented less goods than collection A, and conflict with the definition of goods. Hence when A is chosen B must be at least as expensive, (i.e. $\Sigma\, p_a\, q_b \geqq \Sigma\, p_a\, q_a$). This establishes that the substitution effect is non-negative

$$\text{(by subtraction, } \Sigma\,(p_b - p_a)\,(q_b - q_a) \leqq 0\text{).}$$

Hence we derive both parts of the law of demand from the definition of goods. The hypothesis from which we have deduced it is that goods are goods.

This approach has one drawback (which applies to any approach which allows indifference between collections of goods): it does not guarantee that the consumer will always choose the same collection of goods in a given price and income situation. This difficulty can be overcome by introducing the empirical assumption that consumers are creatures of habit, or by defining goods as objects or services of which the consumer chooses the largest collection he can get.

University of Manchester.

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[1] Kelvin Lancaster, "Revising Demand Theory," *Economica*, New Series, Vol. XXIV, No. 96, November 1957, pp. 354–60. I am grateful to Mr. Lancaster for helpful comments on the first draft of this note, which led to the insertion of the final paragraph.

[2] Cf. Fritz Machlup on "Professor Hicks' 'Revision of Demand Theory'", *American Economic Review*, Vol. XLVII, No. 1, March 1957, pp. 119–35, especially p. 121 bottom.

Statistical Inference

By **Alan Stuart**

The business of a scientist is to infer, from experiment or other forms of observation, the connections between the variables of his system, the directions of their joint variations, and the magnitudes of these variations. When the system and its mode of working become well understood, the scientist can often formulate a theoretical (nowadays usually mathematical-statistical or " stochastic ") model of the system (which will, of course, be more or less approximate) containing a number of critical constants, called its parameters. For example, an economist may be satisfied that a certain mathematical model adequately reflects the underlying relationships in his field of interest. In general, however, it will contain certain constants, such as elasticities, whose precise values are unknown. These are the parameters of the model. The problems of inference are then reduced to those of estimating, and testing hypotheses concerning, the values of parameters, and these are problems of an essentially statistical kind. How are they to be approached ?

To make our discussion definite, we shall confine it to the problem of testing hypotheses, but very similar remarks to the following apply to the problem of estimation. We assume that, as a result of experiment or otherwise, we have a set of observations generated by the system.

Provided that the hypothesis is sufficiently definite, its mathematical consequences can be calculated. Since the system incorporates stochastic elements, these consequences will also be stochastic. We now seek functions of the observations (called ' statistics ') which we may use as the basis of our test of the hypothesis. What properties should we demand of these test statistics ? In some sense, we require that they should furnish the ' best ' test of the hypothesis, but what do we mean by " best " ? At this comparatively early stage in the formulation of the problem, a radical division arises between two schools of thought.

The first school, which was historically the last to develop, and to which the majority of statisticians belong, answers our last question categorically, and answers it in terms of what has now come to be called the theory of inductive behaviour. When we test a hypothesis, it is possible to err in two ways: we may decide against it when it is true, or we may decide in favour of it when it is false. To each of these errors there corresponds a ' loss '. Naturally, we wish to avoid both of these types of error, but with non-trivially stochastic phenomena this is just not possible: any rules for deciding whether a hypothesis is true or false will sometimes lead us into one or other of these types of error. Some compromise must therefore be sought which enables us to minimize our overall loss in making the decision. If we can bring the losses due to the two types of error down to a common unit of measurement, say, a money unit, we can choose that decision procedure (based on the appropriate test statistic) which minimizes (say) the average loss resulting from the decision, and call this an efficient test procedure.

Even this over-simplified account of the teachings of the Neyman-E. S. Pearson-Wald school of statistical inference will be seen to depend on the possibility of evaluating the ' loss ' appropriate to each decision. In Sir Ronald Fisher's latest book,[1] he specifically rejects the idea that such evaluation can be made in scientific enquiries, as opposed to industrial applications, and it is difficult to dissent from his view; indeed, it is often equally difficult to evaluate loss functions in problems of optimum allocation in industry, such as form the subject-matter of much operational research. But we may remove this feature of the situation by reformulating the discussion in terms of the probabilities of error of the two types, as, indeed, Neyman and E. S. Pearson originally formulated it before the work of Wald. Since we cannot now combine the results of the two types of error (because we have no explicit loss functions) we must define test efficiency in some such terms as " that test procedure is efficient which, for a prescribed probability of an error of the first type, minimizes the probability of an error of the second type ".

If we are designing an experiment from the outset, we can use the foregoing theory to calculate the number of observations necessary to ensure that, when basing our decision on the efficient test procedure, the probabilities of error of each of the two types are controlled at any pre-assigned levels. In fact, of the three quantities—number of observations, probability of first-type error, probability of second-type error—two and only two may be fixed independently, so that we can calculate the first from the last two, as just stated. But this also implies that if we have no control over the number of observations available (as is frequently the case in the social sciences) we cannot adjust the probabilities of *both* types of error unrestrictedly: we can fix either one, and this and the number of observations will then fix the other probability of error.

The characterizing features of the view of statistical inference just described are that it operates in terms of decision rules, specified in advance of the analysis, whose properties are expressed in terms of the probabilities of making incorrect decisions if the rules are consistently followed. ' Probability ' in this system is a long-run frequency, applied in this context to the correctness of scientists' assertions. Probably most English members of this school, at any rate, and many of its American members, would agree that the ' decision ' indicated by the statistical analysis is not to be confused with the decision (without quotation marks) made by the scientist, who has to consider the non-statistical ingredients of his situation (including, specifically, the question whether his model is a reasonably close approximation to that situation) and combine them with the statistical analysis before arriving at his own judgment. This last word is used advisedly: the statistical evidence is a witness, whose credentials (probabilities of error) are known, and the scientist has to judge.

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[1] *Statistical Methods and Scientific Inference.* By Sir Ronald A. Fisher, F.R.S. Oliver & Boyd 1956, 175 pp. 16s.

The attitude to statistical inference of the other principal school of statisticians, led by Sir Ronald Fisher, is quite different. Except in rather special cases, Sir Ronald explicitly denies that a test of a hypothesis need have a probability interpretation in terms of long-run frequencies of errors in making decisions. What does he put in place of this frequency interpretation? He regards the probability of the observations, given that the hypothesis tested is true, as a *measure of belief* in the credibility of that hypothesis. ' Probability ' is now essentially a subjective matter: it expresses the scientist's attitude to his hypothesis in the light of his observations.

The distinction between probability as the frequency of correctness of scientists' assertions on the one hand, and as a measure of scientists' belief in their hypotheses on the other, may seem slight to the non-specialist. In practice, indeed, the difference of attitude rarely leads to considerable differences in scientific procedure. In principle, however, the difference is radical, and its implications for modern statistical methods are still stimulating a great deal of research and controversy.

Some of this research has made it clear that Sir Ronald's view of statistical inference is not free from ambiguities, to say nothing of obscurities. He and his school seem to have been concerned to preserve the validity of his original intuitive formulations of the inference problem, but the situation is still obscure and his new book does not greatly clarify it.

It is impossible not to comment on the personal way in which Sir Ronald conducts his scientific discussions. Karl Pearson, an earlier leader of the statistical profession, died in 1936; in this book Sir Ronald still finds it necessary to write bitterly of him. As the greatest living statistician, and indeed the greatest statistician in the short history of mathematical statistics, Sir Ronald would have better achieved his purposes in writing this book by confining himself to scientific issues. But in any case, I doubt whether this would have been regarded as a great book, as are its two predecessors *Statistical Methods for Research Workers* and *The Design of Experiments*.

Mr. Spencer Brown's book[1] is an embarrassing one for a statistician to review, for very different reasons; but reviewed it must be, if only as an awful warning of what happens when the Fisherian theory of statistical inference influences someone with philosophical leanings. Mr. Spencer Brown is concerned with the problem of randomness: it has for fifteen years or more been a well known fact that in a long series of numbers produced by an (efficient) randomising machine, there will be substantial subsets of these numbers which fail to pass any given test of randomness. (The statistician is not at all worried by this. He uses series of random numbers appropriate in length to the purpose he has in hand, and he does so only, in Mr. Spencer Brown's own words, " to save the experimenter from his own dishonesty ".) Mr. Spencer Brown treats this fact as a new discovery, works it very hard, and uses it in conjunction with the interpretation of the probability of the observations, in testing a hypothesis, as a direct measure of belief, to reach some very strange conclusions. He does not discuss the ideas underlying statistical decisions at all, though he gropes for their basis in a passage at the end of his Chapter XI. He does not seem to have heard of the theory of estimation for the parameters of a mathematically specified model—there is no other way in which I can interpret his truly astonishing Chapter XII, 3½ pages long, which contains the modest statement: " Thus totters the whole edifice of Bernoullian probability " and ends with " . . . Bernoulli's theorem " [a valid mathematical theorem] " . . . is one of the most flagrant metaphysical excesses ever to pass as an adjunct to modern science ". To this one can only say something like " Well, well ! " Whatever philosophical merits this book may have, it is of no value to the statistician.

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[1] *Probability and Scientific Inference.* By G. Spencer Brown. Longmans, Green and Co. 1957. viii + 154 pp. 15s.

In his new book[1] Professor Hogben fully shares Sir Ronald Fisher's dislike for the works of Karl Pearson, though he is more polite in his criticisms. His book is an extremely detailed and informative study of the history and bases of statistical inference. His fundamental concern is with the adequacy of the various stochastic models used for the analysis of diverse bodies of data, particularly in the biological and social sciences; that is to say, he dislikes the undoubted tendency to make insufficiently careful examination of the correspondence between the basic assumptions of the models and the real features of the situation being investigated by the scientist. He seems to regard Sir Ronald Fisher as the bearer of the Karl Pearson tradition, and rebuts his all-embracing claims for the domain of validity of statistical inference. It is, indeed, interesting that the Fisherian claims are as strong as those made at the extreme opposite end (i.e., the Wald end) of the school of Neyman-E. S. Pearson-Wald. A further point which Professor Hogben might have made, but does not, is that the intuitive nature of much of the thinking of the Fisherian school has tended to keep its adherents away from modern research into alternative models for the analysis of similar bodies of data (e.g., in the Analysis of Variance, and in the Theory of Regression). The complete absence from his book of any reference to these recent developments or to the related research into the ' robustness ' of statistical procedures (i.e., their insensitivity to departure from the assumptions of statistical models, with which he is so much concerned) and into distribution-free methods of analysis (which are simply defined as almost-completely-robust methods) seriously limits its pretensions to be a complete study of modern statistical methods. It is nevertheless a welcome attempt to set down in one place a coherent attitude to the subject. Its conclusions are firmly in favour of the Neyman-E. S. Pearson school of statistical thought, and apart from one or two idiosyncratic points[1] and a rather journalistic use of the word " crisis " to describe the division of the statistical world into the two schools described earlier, there is nothing in the book from which most members of the majority school would dissent. Professor Hogben attributes a novelty to his views which they do not in the main possess. He is disarmingly naïve in his foreword: [In writing a preceding textbook on statistics] . . . " my own views about widely accepted teaching became more and more critical. . . . My present views are in direct contrariety to many therein expressed in conformity with current tradition." He has, in other words, been led to deeper thought about fundamentals than is common in writers of textbooks. Because he is an extremely able expositor, and keeps even the (very elementary) algebra which he uses to heel, he presents the issues very clearly and will rarely fail to convince, as well as inform, readers new to the complexities of the subject. This is a book unhesitatingly recommended to anyone with statistical interests.

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[1] *Statistical Theory : The Relationship of Probability, Credibility and Error.* By Lancelot Hogben, F.R.S. George Allen & Unwin Ltd. 1957. 510 pp. 45s.

Research Techniques Unit, The London School of Economics.

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[1] For example, Professor Hogben seems to believe that a two-stage decision rule, the first stage of which involves examining a sample in one respect in order to decide whether further observations are necessary to attain a pre-assigned degree of precision, infringes the accepted requirement that decision procedures must be fully specified before examining the observations. But this is not so: the *rule* is determined completely in advance. Again, he points out that statistics textbooks contain no mention of what physicists call " statistics ". The explanation is simple: the physicists' " statistics " is probability theory, not mathematical statistics proper.

Book Reviews

Activity Analysis and the Theory of Economic Equilibrium. By HELEN MAKOWER. Macmillan & Co. Ltd. 1957. 192 pp. 25s. net.

This book, an attempt to re-work the fundamental problems of value theory with the aid of the concepts of activity analysis, is motivated by the feeling that lack of communication from the mathematical economists is leading non-mathematical economists to overlook a powerful new tool of analysis. The missionary motivation is responsible for some of the less satisfactory features of the work: Dr. Makower's enthusiasm sometimes leads her to appear both to caricature " traditional " theory to suit her argument, and to regard anything which cannot be fitted into the framework of activity analysis as *ipso facto* suspect (see, for example, the remarks on the theory of exchange on p. 80). Genuinely non-mathematical economists may also be irritated by Dr. Makower's tendency to lapse into abstruse mathematical jargon after the completion of her excellent arithmetical examples, while the mathematically-minded may be equally put off by her resolute refusal to include any real mathematics. The book should, however, prove both useful and stimulating to the semi-mathematical (and semi-converted), who can acquire from it both a feel for the concepts of activity analysis and—with reflection—a better understanding of theory.

The book is divided into three Parts. The first, and longest, deals with optimal allocation of resources in a Crusoe economy. The method of approach is illustrated by casting a simple farm production problem in activity analysis form, consumption being introduced as an activity using goods in fixed ratios. A more general model involving joint inputs, joint outputs, and joint demand is then formulated; the nature of the solution is described, and analogues of marginal productivity and marginal utility identified, and the dual (value) problem briefly discussed. There follows a discussion of the assumption of constant returns to scale (Chapter VI), which this reviewer at least finds extremely difficult to follow. It is a familiar and reasonable argument that diminishing returns to scale in production are attributable, not to the form of the production function, but to the presence of a fixed factor in conjunction with the convexity of the isoquant (convexity, in its turn, being assumed as an empirical fact or deduced from efficiency or stability considerations). But Dr. Makower argues that the convexity of the isoquant itself can only be explained by the presence of an additional factor whose quantity is fixed; moreover, she extends this argument to consumption as well—in concrete terms, the only way of explaining why it takes more and more extra jam to compensate me for the loss of successive pieces of toast is that the butter on my plate is not increased in proportion with the jam! Further, her treatment of consumption as an activity leads her to ignore the crucial point that the " output " of utility is non-observable, which renders meaningless the question of whether marginal utility is assumed constant or not, but removes any *a priori* reason for assuming that if two activities are indifferent at one scale they will be so at all scales. Chapter VII considers allocation in an open economy, under both competitive and monopolistic conditions.

Part II contains two studies in the non-optimal allocation of resources. Chapter VIII, on the pure theory of taxation, shows the limitations of the technique: the tools do not give the answer (only partly because of the lack of an activity analysis theory of exchange) and Dr. Makower instead resorts to conventional geometry, which is not well presented and contains a number of errors. On the other hand Chapter IX, on the pure theory of specialization (with which goes a geometrical appendix), shows the technique at its best, and would be well worth any economist's reading.

The same can be said of the first chapter of Part III, which deals with some properties of comparative statics analysis in a closed interdependent system: the main point—familiar, but demonstrated here with fresh clarity and cogency—is the inadequacy of partial equilibrium analysis for changes which are not infinitesimally small. The final chapter, which applies activity analysis to the Ricardo effect, is also stimulating though the paradoxes it turns up are perhaps less surprising and less interesting than the author implies, given the initial assumption of a firm endowed with an arbitrary collection of resources.

There remains the problem of assessing activity analysis as an approach to economic theory. On this question, Dr. Makower seems too ready to identify computational facility with inherent superiority. All three of the advantages claimed for activity analysis in the Preface assume that the problem is to compute an answer. The first, clarity (by which is meant concreteness of assumed relationships), considered as an approach to pure theory, persistently raises the question of the general validity of the conclusions; the second, capability " of being interpreted in terms of real things in an actual factory ", is otherwise simply a slander: as descriptions of the process of finding the maximizing solution, marginal adjustments and trial-and-error consideration of alternatives (the Simplex method) make equal common sense. The third, applicability to large changes in data, contains the real point: that such problems require complete knowledge of the functional relationships, which so far can only be effectively handled for computing purposes by the approximations of linear programming. The progress of electric computers and analogues may overcome this handicap, in which case it may seem foolish to have made so much virtue out of necessity. In the meantime, activity analysis does illustrate the logic of certain choice situations, especially short-run resource allocation, more aptly than, and permit the exploration of some interesting problems not easily handled by, more familiar methods. Dr. Makower's book should serve a useful purpose in drawing these virtues to the attention of ' traditional ' theorists.

Harry G. Johnson.

Business Cycles and Economic Policy. By **Erik Lundberg**, translated by **J. Potter**. Allen & Unwin. 1957. 346 pp. 32s.

The subject of this book is not, as its title might suggest, business cycles in general but the growth and fluctuations of the Swedish economy between 1918 and 1955; moreover, considerably more than half of it is devoted to the ten years following the end of World War II. Within this field Professor Lundberg does three things; he gives his readers a clear and concise factual background; he traces the growth of the characteristic methods of analysis which have made it possible to speak of a Swedish ' school ' of economists;

and he applies these methods, together with his own great wisdom and experience, to a review of possible stabilisation policies. Though all three tasks are admirably performed, the third is likely to be of the most general interest to British readers.

Professor Lundberg shows that the Swedish inflation, like that of Britain in the early post-war years, derived much of its initial impetus from abroad. He traces the spread of inflation from sector to sector and gives a highly interesting analysis of the way in which inequalities in the rate of growth of income between sectors may give rise to a cumulative process. This analysis is quite general and might well be applied to a study of the inter-action in the British inflation of wage claims based on productivity, on the cost of living and on fairness in relation to other wages and to profits. Professor Lundberg makes a point that is often overlooked in emphasizing that, if the price level and the share of labour in the national income are to remain constant, wages fixed by collective agreement must rise by less than the average growth of productivity in order to allow for the movement of workers from lower to higher paid jobs and for the element of wage ' slide '. This latter term is used to describe the tendency of earnings to run ahead of agreed wage rates during a boom because of payments in excess of basic time rates, enhanced piecework earnings and overtime.

But what if unions persist in asking and employers in granting wages that are too high by this standard ? A remedy that apparently finds favour with some Swedish economists is to mop up the excess purchasing power by indirect taxation. This is accepted only with reserve by Professor Lundberg, and it would certainly not seem a very fruitful approach for a country such as Britain. Professor Lundberg himself suggests that the state should compress demand by monetary or fiscal means to a point that would bring the necessary stiffening of employers' resistance, and counter any resulting unemployment by discriminatory measures designed to create jobs at selected points in the economy without adding to demand in general. Unfortunately, however, this interesting suggestion is not very fully worked out, and it is not easy to see how these selective additions to demand could be prevented from seeping back into the main stream of spending and again raising its level to a point which would frustrate the original policy.

Professor Lundberg discusses the use as stabilisers of monetary policy, fiscal policy and direct controls, and comes to conclusions very similar to those of most ' middle-of-the-road ' British economists. He has no illusions about the difficulties of using monetary policy, but maintains that they are no greater, and may well be less, than those of ' inflationary gap ' budgeting. The chapter on the latter is a cautionary tale that should be read by all undergraduates as well as by professors and politicians.

E. VICTOR MORGAN.

French Banking Structure and Credit Policy. By J. S. G. WILSON. G. Bell & Sons Ltd., for the London School of Economics and Political Science. 1957. vii + 453 pp. 45s.

In this book Mr. Wilson has set himself the task of giving a detailed account of the structure of the French banking system and a description of credit policy since 1945. The result is that for the first time English and American students have at their disposal an authoritative work which provides the essential background for any critical appraisal of French financial policy.

The French commercial banking system is complex for there are a variety of institutions which do banking business. There are approximately 270 deposit banks which range in size and importance from the four nationalized *grands établissements* with their network of branches over the whole country to very small local banks like the Banque de Savoie and the Banque Courtois. This striking contrast with our highly concentrated banking system is attributed by the writer to such factors as local patriotism and the greater relative importance of small business units which have a natural affinity with their small banking brothers. It would seem that the movement towards greater integration and concentration is likely to be retarded by the nationalization of the four largest banks, for public ownership is regarded as the probable, and not particularly desirable, reward of growth. From the viewpoint of efficiency this is to be regretted.

Before 1914 France differed from her continental neighbours in that mixed banking was not the typical pattern; but during the inter-war period there was a tendency for the activities of both the deposit banks and the *banques d'affaires* to overlap. The legislation of 1945, which made it obligatory for the banks to register either as deposit banks or *banques d'affaires*, halted this development, and the activities of the latter have recently been confined to investment banking. Unfortunately, as Mr. Wilson indicates, their contribution to the modernization and development of the French economy has of necessity been limited by lack of resources and the difficulty of placing new issues with the public in the prevailing atmosphere of distrust in the stability of the franc.

A whole chapter is devoted to an explanation and an analysis of the unique part played by such institutions as the Caisse des Dépots et Consignations in the French money market. This official agency, which administers the funds of the savings banks and many other bodies, lends on a day-to-day basis to the discount houses, accepts short-dated paper from approved banks, and is an important subscriber to new Government issues. Other agencies play their part in improving the marketability of various types of negotiable paper by attaching their signatures.

Part II of the book is concerned with the problems of control and credit policy. Mr. Wilson succeeds in giving a clear picture of the division of responsibility between the three authorities which regulate the banking system. The National Credit Council, which is responsible for general policy, gives advice to the appropriate Minister, but the Bank of France with its extensive powers of qualitative and quantitative control acts as its executive arm. The Banking Control Commission's function is to supervise the different types of banks and, if necessary, to impose sanctions on them.

In the early post-war years the Bank of France relied almost exclusively on qualitative controls which, as in many other countries, had later to be supplemented by quantitative controls. Of the latter the principal one involved the allocation of a rediscount ceiling to each bank in order to limit the extent of its recourse to the central bank for cash. One of Mr. Wilson's most important conclusions is that this method lacks effectiveness if it is not supported by penal charges for any excess borrowing from the central bank. His general verdict is that the Bank of France, although it now has adequate powers of control, has been hampered by the Government's failure to create a favourable environment.

Undoubtedly this book is an important contribution to banking and financial literature, for it places at our disposal the fruits of a vast amount of painstaking and detailed research. But in some ways its emphasis appears to be misplaced. The chief value of studying foreign financial institutions is to provide the necessary background knowledge for a critical analysis of central banking and monetary policies. Mr. Wilson devotes over 250 pages to a description of all the institutions other than the central bank: at times this is so detailed that it is difficult to see the wood for the trees. One feels that some of this space could have been devoted more profitably to the second part of the book. The complexity of the subject obviously raises difficult problems of exposition, but the author's task would have been easier had he been willing to prune his material more drastically.

At one point Mr. Wilson's analysis appears to be misleading. When he discusses the deposit banks' structure of assets (p. 69), he argues that " when the major liability is to pay on demand, it is dangerous to immobilise funds in long-term loans ". Although this view is widely accepted, its validity is doubtful. The unsuitability of long-term loans arises from the risk of eventual loss and not from the nature of a bank's deposits. The only circumstances in which the necessity to give notice of withdrawal would be important would be when a bank was losing deposits to its rivals through a lapse of confidence; and it is precisely in those circumstances that strict insistence on the period of notice would intensify the rate of withdrawal. In the same passage it is surprising to find that lip-service is paid to the principle of self-liquidating assets without any critical reservations.

Apart from these minor blemishes, about which opinions may differ, there is no doubt that Mr. Wilson has performed a valuable service in giving us such a comprehensive and authoritative survey of the banking system of this major European country.

G. CLAYTON.

Political Economy. A text-book issued by the INSTITUTE OF ECONOMICS OF THE ACADEMY OF SCIENCES OF THE U.S.S.R. Lawrence & Wishart. 1957. xxii + 858 pp. 21s.

This 850-page official text-book, the joint work of some ten authors or editors, is issued by the Institute of Economics of the Academy of Sciences of the U.S.S.R., and six million copies of the first edition were circulated. The very proficient and advanced state of the natural sciences in Russia has recently evoked the widest recognition, but if this volume were representative—as surely it ought to be—one could only conclude that the study of economics, and even of Marx, is in a more depressed and backward state in Russia than in any other leading country in the world. Perhaps this book is not fully representative of Russian economic studies. Anyhow, in a one-party or "totalitarian" state, or at any rate in Russia, academic economists have had a completely different role to play from that which they exercise in countries where there is pretty complete or considerable freedom of enquiry, discussion and research. In our British democracy, academic economics, whether or not it contributes much towards wiser or more effective short-run policies, has an invaluable longer-run function in the democratic process, in initiating and setting standards of discussion and enquiry into economic policies by people mostly somewhat detached from business or party interests. In

Russia, apparently, bureaucratic statisticians, for their own purposes, do most of the real enquiry and research into current economic processes and policies, while academic economists have the role of propagating the official ideology, slogans, and OK phrases among the ' intelligentsia ', and of sustaining and codifying the politically correct ignorance and prejudices about the outside world. Academic economists in the West, inclined to hail the success of Russian economic policies, should note that any wisdom or effectiveness they consider that these policies may have shown, can hardly possibly have owed anything whatsoever to any contribution from academic economics —apart from the propagating of ideological unity—judging, at any rate, from this official volume.

It is therefore irrelevant to comment on the crudity of the abstractions and historical over-simplifications in this work, or on a use of statistics, mostly in terms of percentages of unexplained origin, which would secure the immediate failure of a first-year undergraduate in this country. More serious and significant is the inability of the Moscow Academicians to face up to the most elementary economic facts of the modern world. Here are two examples: (i) " Facts, however, prove that under capitalism the workers' standard of living continually declines " (p. 170); and (ii) " Real wages in the U.S.A. and Britain and especially in France and Italy have markedly declined as compared with pre-war " (p. 368).

However, the significance of this work remains as problematic as the internal political future of Russia. This is a translation of the second edition of 1955 and is still an overwhelmingly Stalinist document. One of its principal editors (Shepilov) has since been disgraced, and the book itself was publicly attacked by Mikoyan at the Twentieth Congress. Possibly considerable changes may appear in the third edition already announced.

T. W. HUTCHISON.

Population Theories and the Economic Interpretation. By SYDNEY H. COONTZ. Routledge and Kegan Paul. 1957. ii + 200 pp. 25s.

The failure of contemporary predictions of population trends has inspired the author of this work to attempt to place population thinking on a new basis so that empirical research may become more meaningful. Dr. Coontz writes in an attractive and incisive style, and whatever misgivings one may feel about his general conclusions, there can be no doubt that his book is entertaining and full of provocation, both terms being used in the best sense.

He begins with considered criticisms of such types of theory as appear to rely for their explanatory power upon elements which enjoy an independent or universal existence: biological theories, chiefly of a medical and dietary nature, and cultural theories running in terms of emulation, status, urbanisation, rationalism, and the mentality induced by the reigning organisational principle of society. The discussion of such theories is penetrating and valuable to the student interested in a conspectus of partial theories of population.

It is in the spirit of Marx to reject claims that there is inherent in a particular group of phenomena, biological or cultural, an influence which, on its own, can principally determine population behaviour, for Marx in his polemics against Malthus was attacking just such false universalism. Perhaps most modern scholars, especially in the field of economics, would share this view ; we are all relativists now. But most economists, in the West at least, would probably prefer the discussion, thus liberated, to move to the plane of generalised interdependence.

Coontz, however, argues, and with cogency, that the demand for labour is the true determinant of population growth. It is considered from three leading points of view: aggregate demand, the demand for female labour, and the demand for labour as affected by its own cost of production. Aggregate demand is discussed in terms of the time correlation between the size of wage offers and fertility: general expansion of the system provides the condition of population growth, contraction (or slowing down) provokes the reverse. There has certainly been some tendency, especially on the part of those whose demography is " non-economic ", to overlook this apparently obvious element. But it is taken here as a kind of datum; no attempt is made to explain why it was strong in one phase and weak in another, unless we adopt some sort of rise and fall thesis about capitalism. Moreover, the firm information available on both sides of the correlation is, to say the least, not very ample. In fact, the verve with which Coontz arrives at his conclusions will be rather frightening to those who have struggled to organise scarce and intractable facts in the nineteenth century. The same kind of misgiving occurs over the treatment of female labour. The argument here is that the prevailing mode of production determines the economic function of women; this in turn governs fertility and income, related inversely. For if there is a premium upon women's services over those of men in the ruling state of technology, their incomes will be higher than otherwise, and fertility lower. To emphasise the operation of this kind of influence is important. But much of the historical demonstration here rests on the emphasis placed by Marx on the conditions of the early textile trades, the relative significance of which he much exaggerated. The cost of production theory of wages provokes similar misgivings: " *in general* ", we are told, " in a capitalist society there is a tendency for wages to equal the cost of production of labour power ". But it would surely seem that skilled men receive more than unskilled principally because of the premium on their services through short supply, rather than that wage differentials reflect differences in their respective costs of production. Natural endowment, the flukes of opportunity and education, parental influence, and the like would provide a rent above cost; the difference between skilled and unskilled is not merely one of expense—a process of self-selection is continuously operating, with skilled men becoming such by acts of initiative. The artisans of the new engineering of Nasmyth, Fairbairn, Napier, and the rest, were unskilled men, chosen because they could learn as they earned.

The claim made by the author is that if we look to the conditions governing the demand for labour in the above three senses, we will then be better able both to understand past population growth and to foretell the future. This is hardly enough. If we assume that technology can overcome materials bottlenecks as they arise, and if we place beside demand for labour its twin, demand for income (on which the slighted cultural factors mainly operate), then we should have two categories which could be made exhaustive in dealing with fertility. But to get beyond this, in fact, we need a general comprehension of the economy in its successive phases. To study population does entitle us to lay stress on particular phenomena, such as the demand for labour, which may not be receiving its due. But it is both unscientific and unhistorical to imply that the demand for labour approach exhausts operative reality, for to think with only a portion of the truth may be as misleading as are the mechanistic calculations of demographers.

S. G. CHECKLAND.

Essays in Canadian Economic History. By HAROLD A. INNIS. Edited by MARY Q. INNIS. University of Toronto Press. Oxford University Press. 1956. viii + 418 pp. 68s.

The twenty-eight essays in this book are a selection from the voluminous writings of the late Professor H. A. Innis, the great scholar who left no school but whose thought has profoundly influenced a generation of Canadians. It is hardly too much to say that, in an economic sense, Canadians' understanding of themselves derives directly or indirectly from Innis's penetrating analysis of Canadian economic history. The committee responsible for selecting the essays had to choose from an embarrassment of riches, but it " believes that apart from one or two essays which were excluded solely because of length the essays here published represent the best of Innis's scattered writings in Canadian economic history ".

Innis's essays all make very difficult reading. It is perhaps significant that in the Foreword no attempt is made to outline or to appraise the material in the text. The difficulty inherent in forcing one's thought out of old ruts in order to follow an original thinker is in this case compounded by a style that presents many difficulties of its own. Innis's prose at its best had the truly prophetic quality which expands one's insight in a startling, explosive manner; at its worst syntax and vocabulary buckled under the strain imposed by his eager mind. Understanding him is never easy; he would no doubt have pointed out that in political economy understanding is never easy. How he exerted such a great influence with such a gnarled style is a major conundrum. With respect to his written work the answer is that the originality, the importance, and the insight of his generalizations shone through his style and reduced it to an insignificant irritation. With respect to his oral impact in lectures the mystery remains; understanding—of human dignity as well as of economic history—came in through the pores.

The present volume is a memorial to Innis, but it would be a pity if a reader were introduced to him through these *Essays*. He should be approached through *The Fur Trade in Canada*—commonly judged his most important book, probably because it is the easiest to read—and *The Cod Fisheries*. The *Essays* will then prove less baffling. In them, Innis is in part struggling and in part playing with the material he knew so well. He is grappling for relationships that will make some sense out of the chaos of historical fact, looking for continuities and explanations of discontinuities, casting about for propositions from economic theory that will illuminate historical trends, and casting doubt on the relevance to new countries of economic theories worked out in old countries.

The title of the book is appropriate, but it should be understood that, to Innis, Canadian Economic History was a very broad field. Canadian history in his hands was the history of new areas, and especially the history of the relationships between new areas and old areas—not only the impact of Europe on the New World, but also of the New World on Europe. For example, the essay curiously entitled " Liquidity Preference as a Factor in Industrial

Development " is an important contribution to imperial history. It is a study of the spread of industrialism to new regions in the two decades after 1850 when " Commercialism began to give way to capitalism ", and of the adjustments that ensued both in the new regions and in Great Britain. Canadian history also raised in its most acute form the problem of relations with the American economic colossus. It need hardly be added that what has long been a special Canadian problem has now become a problem for many countries. Innis did not, perhaps, throw much new light on the shifting strands of the relationships between Canada and the United States, but the essays on " Economic Trends in Canadian-American Relations " and " Great Britain, the United States, and Canada " show that he was at his pungent best in discussing them.

If Canadian history to Innis was much broader than the history of Canadians, Economic History was almost universal. To him it was no narrow history of economic life, but a study of the fascinating complexity of political economy. His search for the linkages between politics and economics took him to diplomatic history, business history, sociology, and personal biographies and reminiscences. In a very broad sense it was the search for these linkages that led him to that study of communication and the carrying agents in a people's culture which consumed the last decade of his labours. Intimations of this later interest appear at only a few places in the *Essays*, but every page of the volume bears witness to the breadth of his scholarship.

J. H. Dales.

South Africa: Economic and Political Aspects. By H. M. Robertson. Published for the Duke University Commonwealth-Studies Center by the Duke University Press, Durham, N.C., and Cambridge University Press, London. 1957. x + 192 pp. 26s.

This scholarly little book by the Professor of Economics in the University of Cape Town is a welcome change from the current avalanche of pseudo-scientific and journalistic outpourings on Africa. It consists of three essays based on a series of lectures delivered at Duke University in 1956 which have been revised and expanded by the author. The essays are written around the central theme of the experiments in race relations going on in South Africa. The first deals with the subject of " Black and White in the South African Economy—the Historical Development ". The second is entitled " Apartheid and the South African Economy " and the third discusses " The Recent Constitutional Struggles and their Background ". The book also contains an unusually valuable glossary, a bibliography and an index. Those who are aware of Professor Robertson's writings will know of his exceptional qualifications as historian, economist and political scientist to write from these points of view. The book is much more than a collection of essays and I can recommend it as one of the best introductions to the problems of South Africa of which I know—if only because of the delightful style in which it is written and the impartiality which characterises its approach.

The South African experiment in race relations has been going on for some three centuries, and only a writer versed in the multifarious turnings of South African economic history can adequately portray it. After reading this book the reader will, I believe, approach the problems of South Africa in a new and chastened spirit.

Professor Robertson gives a finely balanced account of the ideology of Apartheid. He shows that the plans proposed by the Tomlinson Commission to arrive at a just solution on the basis of that ideology are very radical in some respects and very tentative in others. In consequence, he concludes, one of the dangers to the South African society and the economy " is that while the difficulties, real or political, in the way of rapidly creating a separate Bantu economy cause a standstill " in the necessary rate of active development which the Apartheid policy really requires, " yet more rapid progress is made with the other side of a legislative and administrative programme, based upon the premise that the Africans (and for that matter the Cape Coloured People and the Asians) have no part in a common society or in the economy controlled by White South Africans, and at some time in the future will instead enjoy the benefits of their own separate ones. That is to say, the restrictive side, which does away with common citizenship and segregates from common opportunities, is easier of accomplishment than the creation of adequate new forms of citizenship and of separate economic opportunities for the Non-Europeans, which will be superior to those forgone. Thus increasing degrees of separation, involving immediate disabilities, may be brought about at once, while separate development creating a higher range of opportunities in a segregated society, may have to wait till the more distant future, or be indefinitely postponed."

A society is in this like the individual who, as Henry Ford said long ago, is either going forward or backward but who cannot stand still. Apart from the ideological aspects themselves it is time, in my opinion, that South Africa took note of the costs of standing still—for that I believe is the real implication of Professor Robertson's warning. In my view the cost of Apartheid in terms of the relative poverty and avoidable human suffering which it involves is the Achilles heel of the whole experiment.

This little book is in fact the intelligent man's guide to the economic, political and constitutional struggle in South Africa, and an excellent reference book into the bargain.

S. HERBERT FRANKEL.

International Economic Papers No. 7. Translations prepared for the INTERNATIONAL ECONOMIC ASSOCIATION. Macmillan and Company Limited. 1957. 184 pp. 15s.

This is the seventh volume and seventh year of this important publication, bringing to English-speaking economists translations of important articles from various European journals. This number has nine in all. Two by Sismondi, 1820 and 1824, are of merely historical interest but make fascinating reading, He lines up with Malthus against Ricardo and Say, the latter's " Law " receiving no mercy, and we are reminded of a more recent school of thought when we are told that the pyramids did after all serve a useful purpose. Did they not give employment to the Egyptian masses who, but for this merciful invention of a priestly caste, must have died in the midst of plenty from an over productive agriculture and an insufficiency of demand ? Einaudi on the " Theory of Tax Capitalization ", 1928, and Gibrat on " Economic Inequalities ", 1931, are of contemporary interest despite their pre-war dates. The remaining five articles are recent and of variable quality. Lindahl on " National Accounting " stands out as the highlight of this volume and his method of compromise over the basic theoretical difficulties is convincing, while Kragh on " A Basic National Budgetary Model " is clearly also a contribution of importance in this field. Brus, " Socialist Production and the Law of Value ", and Lange, " Outline of a Reconversion Plan for the Polish Economy ", throw some light on theory and practice in the application of Marxism to the planned economy, but both theory and practice have a somewhat woolly look. The volume concludes with a pretty piece of analysis by Giersch on " The Trade Optimum ". This number maintains the high level of its predecessors and it is to be hoped that the International Economic Association will continue with the good work of producing bigger or more frequent volumes for the benefit of that large section of English-speaking economists who are virtually monolingual.

W. E. Armstrong.

Industrial Trade Associations. By P.E.P. George Allen & Unwin Ltd. 1957. xiii + 340 pp. 30s.

" Trade associations ", states the dust cover of this book, " are now an important part of modern industrial society. But by comparison with trade unions and firms they have received little comprehensive attention ". P.E.P., by presenting this survey of the facts, has gone a long way towards filling the gap.

The Committee on Trusts in 1919 estimated that there were then more than five hundred associations of manufacturers. There are now, according to this report, some 1,300. A register, covering 52 pages, of all known associations grouped according to the Standard Industrial Classification, provides a useful guide, and a glance through it gives an idea of the pervasiveness of this type of organisation. Associations cover most branches of manufacturing industry, and range in size and importance from the British Iron and Steel Federation or the Society of Motor Manufacturers and Traders to the Shoe Tip Association or the British Baby Carriage Manufacturers' Association.

Part I of the study surveys the history and structure of associations, and discusses various reasons for their growth. Although a good deal is said about it, perhaps not quite enough emphasis is placed on the influence of increasing participation of government in industrial affairs, especially during the last war and the immediate post-war years. It is significant here that great stress is laid by the associations and by their members (and by this Report) on their importance as representatives of their industries in their relations with government departments. We have indeed moved a long way from Adam Smith's position that " though the law cannot hinder people of the same trade from sometimes assembling together, it ought to do nothing to facilitate such assemblies; much less to render them necessary ". He was, of course, thinking primarily of the emergence of " contrivances to raise prices ". Price agreements are sometimes part, but only part, of the activities of associations. P.E.P. estimates (on what appear to be reasonable assumptions) that the prices of some 240 products are controlled by associations, about one-half being subject to fairly rigid agreements. The picture here may well be changed as a result of the Restrictive Trade Practices Act of 1956 and for this reason the Report says less on this matter than the material available would justify; a further study may be undertaken when the Act has been in operation for some time. It remains to be seen whether the recent legislation can purge associations of some of their less admirable practices and leave them free to pursue their more beneficent ones.

These latter cover a wide range, and Part II of the Report, comprising about one-third of the text, is devoted to this aspect, with sections on the different forms and methods of representation and organisation of common services (and also on price regulation). Part III deals with the structure of associations and the various forms of organisation. Parts of these chapters make rather dull reading, in that there is a tendency for description to become a series of accounts of individual associations; but the general picture emerges clearly enough.

An interesting short chapter (in Part IV, " The Attitude of Member Firms ") presents the results of a sample enquiry among small firms, 626 of which provided information of their membership of associations and the use they make of them. It appears that the greatest use is made of, and value put upon, representation to government departments and other bodies (among which the British Standards Institution comes high), the circulation of business enquiries and general market information, the provision of technical information, and individual assistance in interpreting government regulations and providing export information. It is interesting to recall that Marshall, in the *Principles*, suggested that collective services of this kind might be of growing importance in the continued survival of small firms. The provision of information about costs and prices is also stressed, and this may reflect the importance of price-fixing agreements. A brief survey of the part played by very large firms (including the C.W.S.) suggests that they receive less benefit than the small firms, that " in voluntary work and expert knowledge " they contribute more than they get, and that they participate largely on grounds of public spirit.

S. R. DENNISON.

The Economics of Nuclear Power. Edited by J. GUÉRON, J. A. LANE, I. R. MAXWELL, and J. R. MENKE. McGraw-Hill Book Co. Inc., New York. Pergamon Press Ltd. 1956. xii + 513 pp. £6.

This first volume in Series VIII of an eight-series project titled *Progress in Nuclear Energy* consists of some forty-one articles and essays divided into five sections. There are thirteen articles relating to energy needs and resources of the world and of selected countries, five concerned with nuclear power costs, seven devoted to problems of nuclear fuel cycles, four concerned with economic aspects of various reactor types and reactor programmes, and twelve relating to problems of administration and law. The economist probably will find his main interest in sections two and four, while the political scientist may prefer the section devoted to administrative and legal problems. Two short forewords are provided in a bilateral British-American approach —one by Sir Claud Gibb and the other by Mr. Philip Sporn, President of the American Gas and Electric Company.

The majority of the articles appear to be reprints, in whole or in part, of papers presented at the August, 1955, International Conference on Peaceful Uses of Atomic Energy, Geneva. Those appearing in the section devoted to needs and resources contain some, but not all, of the reports solicited by the United Nations from member states. A more complete UN summary was published in March, 1957, but was not available when this volume went to press. Some of the Geneva papers have been revised or slightly modified, and some half dozen were written or presented after August, 1955. From internal evidence it is not clear to what extent parts of the Geneva papers were omitted. Without having made a detailed comparison, the reviewer's impression is that almost all have been cut.

Space does not permit a discussion of each article but a few are of special interest. For example, contrasting " An Evaluation of Geneva and Post-Geneva Nuclear-Power Economic Data ", by James A. Lane, with " The Graphite-Moderated, Gas-Cooled Pile and its Place in Power Production ", by Sir Christopher Hinton, clearly indicates the basic difference between American and British approaches. American reactors rely heavily upon enriched uranium as a fuel in contrast to British use of natural uranium. The former, of course, is the result of processing in American plants while the latter is widely available. Sir Christopher asserts that this difference in choice of fissile fuels is largely due to geographical circumstance. In his introduction he writes, " If Great Britain had not been such a densely populated country and we had easily been able to find sites where safety distances could be met, we probably should have followed the American lead and built graphite-moderated, water-cooled reactors " (p. 323). At the conclusion of his paper, however, Sir Christopher likens the gas-cooled reactor to the reciprocating steam engine—a sturdy, stalwart, economical machine widely used for two centuries. It is no secret that many American scientists and engineers consider the gas-cooled type a rather plebeian Model T, soon to be relegated to an atomic energy historical museum. Some of the present and future competition for foreign markets by British and American reactor producers may revolve around this dispute.

To the reviewer (an economist by profession), the most fascinating article is " The Cost of Power and the Value of Plutonium from Early Nuclear Power Stations ", by J. A. Jukes. Under the British programme, the question of whether or not electricity from nuclear sources is economically competitive with that produced from fossil materials depends in large part upon the value or price assigned to a reactor by-product: plutonium. At present the major use is for weapons although, in the future, it may be used as a nuclear fuel. There is no market for it, no market price, no objective measure of its value and many cost details are secret. The value or price assigned, therefore, is to some extent arbitrary and, under such circumstances, one cannot be certain whether resources are being used in an economic way or perverted. As Jukes states the problem: " The question of the price of plutonium is not therefore academic. Even though in practice the market is artificial, we must try to assess what the price would be in a free market, which should represent the true economic value." And, as if this were not problem enough, " the figure required is not merely the proper price at the moment, but the average price over, say, the next 20 years. . . ." (p. 189). In view of the fact that not only the United Kingdom but several European countries as well are determined to go the " plut route ", the questions Jukes raises are likely to be of crucial importance for more than a decade.

" Post-Geneva Economic Aspects of Nuclear Power ", by Jules Wayne, a reprint of an address given at a meeting of the Atomic Industrial Forum at Washington, D.C. in September, 1955, deserves special mention also because the author expressed some healthy skepticism concerning costs at a time when the majority of estimates were over-optimistic. After reviewing general cost considerations and particular problems of under-developed countries, Mr. Wayne concludes that " unless the economic aspects of nuclear power are more realistically evaluated many countries may find they are employing scarce technical and scientific personnel and capital for purposes which offer only very remote advantages " (p. 348). Considering some of the cost developments since that was written, one cannot but admire the author's intelligent foresight.

E. L. Hollis' article, " The United States Atomic Energy Act of 1954—A Brief Survey ", gives a partly erroneous statement of United States policy in the Atomic Energy Act of 1946. He writes that, " In 1946 the American Congress decided that atomic energy should be developed as a government monopoly surrounded by a wall of secrecy ". Further, Hollis infers that " the policy . . . was to some degree the result of the desire to maintain America's international monopoly " (p. 495). At best this assertion is only a part of the truth. Atomic energy did begin as a government monopoly and for some time seemed destined to remain so. But the major consideration of Congress in writing the 1946 legislation was the hope that an agreement could be reached among major nations to outlaw the employment of atomic energy for destructive purposes. Congress specifically stated that no international exchange of scientific and technical information on atomic energy for industrial purposes could take place until Congress, by a joint resolution, had declared that an effective international agreement for the control of atomic energy had been reached. The Congress expected inspection and control of fissionable materials to be major instruments for enforcing the hoped for agreement. Perhaps the desire for agreement was naïve, or even unwise, but the motivations were not selfish nor crass.

The principal weakness of the volume lies in the editing or absence thereof. Included as one of the articles in the needs and resources section is part of a Soviet Geneva paper by D. I. Blokhintsev and N. A. Nikolayev, " The Comparison of an Atomic and a Coal-Fired Electric Power Station ". Yet when this is referred to in other articles (cf. pp. 170, 182, 207), there is not even an editorial footnote to inform the reader that, in fact, the original article is available only a few pages away. In one instance an article summarizes by paraphrase the immediately preceding article the reader has just finished. Motion pictures are said to employ a division of labour—cutting and continuity are not always performed by the same person. The cutter was certainly at work on this volume but there is little evidence of continuity.

Despite this drawback, and the high attrition from obsolescence suffered by some of the articles, the volume is a valuable contribution for the economic reference shelf. It is indeed a welcome relief from the flood of Sunday supplement ' treatises ' on atomic energy.

ARTHUR KEMP.

The Demand and Supply of Scientific Personnel. By DAVID M. BLANK and GEORGE J. STIGLER. National Bureau of Economic Research Inc., New York. 1957. xix + 200 pp. $4·00.

The 1950 Census reported that there were in the United States 534,000 engineers, 76,000 chemists, and only 72,000 other professional natural scientists, of whom (surprisingly) the largest group were the statisticians, numbering 19,000. The corresponding British figures for engineers and scientists *in employment* in 1956 were: engineers 68,500, chemists 20,700, other natural scientists 30,500: but it should not be supposed that these figures are closely comparable to the American data. If the U.S. Census is to be believed, the number of engineers in that country rose by over 80 per cent. between 1940 and 1950; and although Messrs. Blank and Stigler do their best to explain where this increase could have come from, a suspicion remains that the increase has been assisted by the same social trend which has turned the dustman into the refuse collector, and the ratcatcher into the rodent operative.

In fact, this is tricky ground for the statistician. What Dr. Blank and Professor Stigler have tried to do is to assemble the available statistics on the numbers of scientists and engineers, the use made by different industries, the inflow and outflow, the salaries and prospects, and to see how far various methods of analysis of the imperfect data yield meaningful results. The British observer can only wish that he had so many facts to use, and admire the skill with which the authors fit them together.

Relative to other groups in the population, U.S. engineers show falling relative earnings; they have done even worse than college teachers. There is no evidence that this decline in economic status is due to imperfect mobility or to combination among buyers of their services. The conclusion follows that there has been no shortage of engineers. This seems, to the outside observer, to require rather more analysis than it receives in the book, if only because of the strong sociological forces which affect the levels of salaries. But it is at least plausible that an occupation which expands so fast, despite falling relative earnings, is subject to influences which may produce a surplus.

The increasing demand for engineers and chemists is partly a consequence of a change in the pattern of industry which has favoured trades using much technological personnel. To a small extent only, the change is due to the net effect of the increase in Government research. But more than half is left to be explained by the substitution of professional for other workers, or by technological change, or by statistical error. Precisely what has happened remains uncertain. On the supply side, it is shown that the salary expectations of an engineer still justify with ease the extra cost of college training, and that a moderate further expansion of college-trained engineers should be possible without a serious decline in average intellectual quality. On non-graduate engineers, the data are vague.

A final chapter analyses the demand and supply for mathematicians and physicists in college employment, confining itself to those (about 5,600 in 1951) with a Ph.D. There is probably to be, by 1965, an increase in U.S. college enrollments of around 75 per cent. above the 1954 record level—a prediction which should give rise to thought on this side of the Atlantic; and, since the ' faculty to student ' ratio is (perhaps) stable and the proportion of teachers with doctorates may increase, the expected requirement for mathematics and physics teachers with the Ph.D. would be at least 75 per cent. higher by 1965. On the supply not much can be said, except that it will itself rise, after a time-lag, with college enrollments. Whether colleges will find the finance to expand at the required speed is another matter, and is not discussed; ' demand ' and ' supply ' in this chapter are essentially requirement and possible output, without reference to price.

It is time that the United Kingdom devoted more scholarly care to the study of the market in scientific personnel.

C. F. CARTER.

Bulk Buying from the Colonies. By CHARLOTTE LEUBUSCHER. Oxford University Press for the Royal Institute of International Affairs. 1956. x + 206 pp. 30s.

Long-term bulk purchase agreements between governments are sometimes advocated as a measure for promoting the economic development of colonial territories or as an instrument of national planning. Dr. Leubuscher's study of the wartime and post-war agreements between the United Kingdom and the colonies will give little comfort or support to those who hold such views. Agreement prices which are substantially different from free-market prices are liable to have adverse economic effects. Moreover, " experience during the fifteen years of British bulk purchase has shown that neither sellers nor buyers are willing to acquiesce, over a long period, in prices which are considerably less favourable than those received, or paid, by other sellers or buyers " (p. 99); this attitude led to the introduction of greater flexibility in pricing, which brought contract prices more closely into line with market prices, or to the " premature termination of many of the agreements " (p. 118). In practice, then, this type of long-term contract does not work, or is not allowed to work, as its present-day advocates want it to work. Dr. Leubuscher's enquiries admittedly relate to a disturbed period in which the initial agreements were improvised hurriedly; but she makes it clear that the intrinsic difficulties would be present, probably in an intensified form, in more " normal " conditions. And one is led to the conclusion, though the author does not state it specifically, that the only long-term bulk purchase agreements which have a healthy expectation of life are those which would leave both parties in much the same position as if they had no agreements at all.

All this is well illustrated by references to particular agreements and incidents. In addition, there is useful discussion on a variety of topics, including, for example, the effects of bulk buying on quality of product, on processing in the colonies, on the organisation of commodity trading, and on price fluctuations in the open market. Dr. Leubuscher also considers the discrimination in the treatment of different groups of producers, and the political difficulties which arose occasionally. The colonial marketing boards and their policies necessarily come in for some discussion, but this is mainly in the form of marginalia.

Though this does not affect the validity of the main theses in the book, several of Dr. Leubuscher's observations cannot be accepted as they stand. Sometimes this may be because of the brevity of statement. For example, it is not clear why the Nigerian Oilseeds and Cotton Marketing Boards should have had an " even stronger motive " than the two cocoa boards " to withhold part of the [sales] proceeds from producers to build up reserves " (and therefore were " even more cautious in fixing producer prices ") because during the boom they received comparatively low agreement prices while the cocoa boards sold freely in a sellers' market (p. 95). The validity of this line of reasoning depends largely on the objective pursued by the boards. If the purpose was the establishment of reserves solely for the stabilisation of producer prices then, *cet. par.*, to the extent that the oilseeds and cotton boards received less than market prices in the boom years, a relatively lower average " stabilised " producer price was attainable, and hence relatively smaller reserves were called for. Relatively larger reserves would have been necessary only if the prices in the long-term contracts were expected to be less stable than market prices, implying, in the context, the expectation that agreement prices would be even further below market prices in bad years than in good.

B. S. YAMEY.

An International Trade in Managerial Skills. By J. S. FFORDE. Basil Blackwell. 1957. viii + 153 pp. 18s. 6d.

Of the making of books about under-developed countries there is no end. Such countries are naturally short of skilled managers, and they need to import them. The subject of this book is how the import can best be arranged, and Mr. Fforde has gathered information upon it by oral enquiry among those who have had experience of it in Britain, as the exporter, and in India, Pakistan, Burma and Indonesia, as importers.

Was this book really necessary? Mr. Fforde has some doubts about it himself, as a reading between the lines of his concluding chapter may show. There are of course some interesting problems in this trade in skills, as there are for that matter in the recruitment of skills within a country. But even though the recruitment of a manager by an enterprise in, say, India from, say, Britain presents problems over and above those of recruitment within Britain, Mr. Fforde might well have satisfied the needs of the International Bank for Reconstruction and Development, which sponsored his study, in a handy 10 or 20 page report. In order to produce a book he has had to embark on needless elaboration and analysis, and make a great fuss about many things that are matters of common sense.

Sometimes managerial skills are best imported by individual recruitment. And sometimes it is best to make use of the managers who can be supplied under a variety of arrangements by foreign (i.e. ' developed country ') firms or organizations. That is really all there is to it.

Though the reader soon perceives that this book is much ado about very little, it is easy and pleasant reading; and in passing, Mr. Fforde makes many shrewd, interesting and valuable observations about relations between developed and under-developed countries. His chapter on the British trader-industrialist and agency house is in fact a useful little piece of descriptive economics in its own right.

A. A. SHENFIELD.

The Measurement of Levels of Living with Special Reference to Jamaica. By C. A. MOSER. Colonial Research Studies No. 24. H.M.S.O. 1957. 106 pp. 13s. 6d.

In recent years considerable stimulus has been given to the consideration of problems of measuring levels of living by the discussions within the United Nations Social and Statistical Commissions and the Specialized Agencies. The difficulties are now generally recognized to be formidable and it has become clear that further progress is dependent on experience gained in practical attempts to measure levels of living in various parts of the world. The Colonial Economic Research Committee are therefore to be congratulated on sponsoring an independent enquiry on this subject in Jamaica by the Reader in Social Statistics in the University of London. This slim book represents Mr. Moser's report on his work in Jamaica during a visit of nine weeks in 1955.

The report is divided into four parts. In Part I the author discusses briefly the various concepts of levels of living and the approach to his task. Part

II contains the main body of the report and includes a detailed consideration of those components of the level of living that he selected for study. There is a brief discussion of problems of methodology in Part III, and Part IV contains some recommendations for further work.

The United Nations proposed 12 " components " of the level of living and a large number of " indicators " as measures of changes in these components. Mr. Moser is at pains to emphasize that owing to the briefness of his visit he had necessarily to be highly selective in his choice of components and of indicators. He was anxious to avoid purely economic measures of progress and to concentrate attention on social measures which are often given less priority in research work. He limited himself therefore to four components, viz.: nutrition, education, health and housing—not by any means the easiest. It was of course in the choice of indicators that the real problems emerged. Mr. Moser was concerned to choose indicators that were appropriate to Jamaica and to discuss the indicators that should be used rather than those that were available. He makes the important point that wherever possible one should aim to go beyond indicators for the country as a whole and obtain information about distributions which will enable comparisons to be made between the worst-off and the better-off sections of the community.

Mr. Moser is firmly on the side of those who believe that while individual indicators have meaning, the combination of indicators and of components into indices has not. He suggests a useful rough classification of indicators into (*a*) indicators of resources, (*b*) indicators of utilization of resources and '(*c*) indicators of the end-effect at which these resources are aimed, and argues that there can be no point in attempting to combine indicators relating to resources with those relating to utilization or end-results. On the other hand he suggests that an attempt should be made to analyse the relationship between indicators rather than to treat them only as independent series. He warns against the dangers of international comparison, except between countries and territories in which patterns of living are similar, and urges that if such comparisons are made the measures of levels of living should be set against appropriate yardsticks or bases of references in each country. In his recommendations for future work he argues that priority should be given to series needed for internal comparisons—both over time and between regions and population groups—rather than for comparisons between countries.

In view of the various limitations with which he was faced Mr. Moser was forced to confine his comments in Part II on the indicators used to their accuracy and coverage and to the extent to which they illuminate changes in the levels of living. Although he brings together a good deal of very interesting statistical information under the four headings of nutrition, education, health and housing, Mr. Moser's main object in this principal section of his report was to examine this material critically in relation to the objective of measuring levels of living. In the reviewer's opinion Mr. Moser's report is a model of what such a study should be. It is short, well written and stimulating and avoids none of the difficulties. It is much to be hoped that it will be widely read not only by those whose practical concern is with the problems of measuring levels of living but also by those who want to understand the kind of contribution that the statistician can make to the solution of these problems.

R. F. FOWLER.

A Course in Multivariate Analysis. By M. G. Kendall. No. 2, Griffin's Statistical Monographs and Courses. Charles Griffin & Co. Ltd. 1957. 185 pp. 22s.

This book is based on lecture notes of courses given by Professor Kendall in the United States and in England in the years 1954–55. It is designed to help those students of statistics who wish to tackle the type of problem in which multivariate analysis is appropriate, but do not wish to " plumb the depths " (or " scale the heights " ?) of the mathematical theory involved. Nevertheless, as the author points out in his preface, a substantial amount of mathematical and statistical knowledge is an essential prerequisite to a full understanding of this book. It represents, however, a successful attempt to impart a practical flavour to this subject, which has sometimes been used as an excuse for mathematical exercises of a rather useless nature.

Classification of subject matter has long been a major difficulty in multivariate analysis, which includes a bewildering variety of problems and techniques. Professor Kendall has produced something like system out of near-chaos, though exception might be taken to the order in which he chooses to deal with his subjects. These are: component analysis, factor analysis, functional relationship, canonical analysis, " some problems of sampling ", history, tests on homogeneity and discriminatory analysis. The introduction of history in the middle of the course seems rather odd. Apart from this, it might be claimed by those disliking factor analysis and associated techniques that the topics are arranged in increasing order of usefulness; this would, however, be by no means a universal attitude.

The book concludes with a useful bibliography and a set of seventeen theoretical exercises (without solutions). There is no index, but this is not an important defect in a work essentially consisting of lecture notes on a succession of fairly well defined topics. The text is in typescript, and slightly troublesome to read, but the book is of a handy size, and reasonably priced, considering its specialist nature.

N. L. Johnson.

Studies in Income and Wealth. Volume 21. *Regional Income*. National Bureau of Economic Research. Conference on Research in Income and Wealth. Princeton University Press. Oxford University Press. 1957. x + 405 pp. 63s.

Except for some of the wilder Celtic elements (who are rather odd birds anyway, of course), contemporary British economists do not show much interest in regional economics. This volume is proof enough of the contrast between the U.S. and the U.K. in this regard. It comprises ten papers prepared for the Conference on Income and Wealth held by the National Bureau of Economic Research in 1955, plus comments by some 25 other knowledgeable persons. One doubts very much if a comparable percentage of the professional economists of the U.K. could be mustered for a discussion of regional income measurement. Perhaps this is because of the higher degree of geographical integration within the U.K., although to some extent this must be offset by the smaller degree of factor mobility which we are led to believe exists on this side of the Atlantic; perhaps it is merely because our transatlantic colleagues are often quicker off the mark than we are.

Those of us who have tried to grapple with the fascinating problems of regional income measurement in the U.K. (and, dare I say it, a good many others who have not) will find this volume immensely interesting, encouraging and useful. This is particularly true of the first four papers, which are devoted to issues of general principle; the succeeding six papers (commencing with a truly Olympic effort by F. A. Hanna which, together with its comments, covers no less than 80 pages) are concerned more with empirical issues in which the experience of the U.S. naturally bulks largely. The four conceptual papers are in general of the high standard one has come to expect from this series; the familiar problems of regional analysis are discussed thoroughly and intelligently—the dilemma created by geographical regions which make little sense economically and economic regions which have neither permanence nor official statistical existence, the awkward character who lives in one region and works in another, the equally awkward consumer who insists on shopping by post outside his proper region, and so on.

Good though these discussions are, I must confess to some slight feeling of disappointment on two scores. The first is that what sets out to be a discussion of conceptual issues degenerates rather frequently into one of the purely statistical difficulties involved in regional analysis ; the disappointment was the more acute because the tendency was especially marked in the first paper included in the book and entitled ' Conceptual issues of regional income estimation '. The second rather disappointing feature was the relatively very small amount of space devoted in the volume to what British workers in this field would regard as the two *bêtes noires* of regional income measurement—the regional allocation of corporate profits and the treatment of the central (trans. Federal) government. On the former, Hochwald and Perloff more or less throw up their hands, although neither gives any discussion to the possibilities of estimating regional corporate profits from census of production figures of net output rather than from tax returns and company accounts. On the latter, which is especially thinly treated, Hochwald—the only one to address himself even briefly to the problem—argues that if one applies to regional analysis a treatment comparable with that adopted for the national accounts it ' would involve a highly provincial philosophy of government '. I am surprised that not one of his three commentators took him up on that.

Nevertheless the volume as a whole is a most useful and informative collection of views and experience in a branch of social accounting which is still in its infancy. If Oscars have to be given, mine would go, I think, to Walter Isard's most stimulating contribution ' The Value of the Regional Approach in Economic Analysis ', and the illuminating comment on it by E. P. Weeks of the Canadian Department of Public Works. I hope offprints of this will be widely circulated in the U.K.; I know of a good many people, especially in official circles, who could read it with great profit.

EDWARD NEVIN.

Studies in Income and Wealth. Volume 20. *Problems in the International Comparison of Economic Accounts.* NATIONAL BUREAU OF ECONOMIC RESEARCH. Conference on Research in Income and Wealth. Princeton University Press. Oxford University Press. 1957. x + 404 pp. 63s.

This book is the twentieth volume resulting from the annual labours of the Conference on Income and Wealth in the U.S.A. Readers of previous volumes will have come to expect a solid foundation of statistical material and a high level of technical competence from this series, and they will not be disappointed on this occasion.

The theme of this Conference was the difficulty of making international comparisons of economic accounts, and an impressive list of American economists contributed papers or took part in the discussions. The papers fall into two main categories, one being concerned with the elaboration of existing techniques of economic accounting, and the other with the consideration of theoretical and conceptual problems.

In the first category, Copeland writes on " The Feasibility of a Standard Comprehensive System of Social Accounts ", Woolley on " The Elaboration of a System of International Transaction Accounts " and Brady and Hurwitz on " Measuring Comparative Purchasing Power ". The first paper is a detailed elaboration of the U.N. " standard " system of accounts. Whilst one must admire the dexterity and persistence of the author in juggling with all conceivable sorts of transactions and making all the right rabbits go into all the right holes, one has a feeling that he never really stops to ask himself why he is doing his juggling, who will benefit from it or what the point of it really is. Devotion to a cause is a fine thing, but an open mind and a willingness to ask oneself all the time, " Is this really sensible ? And is it really a priority ? " is even better. To some extent the same comments apply to Woolley's paper, but because this field is less well tilled one does not have quite so many qualms about it. Mrs. Brady's paper is, as one might expect, ingenious and penetrating, but one does get the feeling that on this occasion she and her co-author have been over-ingenious. The paper does not really stand up to the trenchant criticisms ventilated by Professors MacGregor and Tobin in the discussion.

The second category of papers consists of two linked papers by Gerhard Cohn and Marilyn Young on " The Government Sector " and another by Kravis on " The Scope of Economic Activity in International Income Comparisons ". There is a great deal of thoughtful work in Cohn's paper and in the discussion of it. One of the less satisfying point, however, is the discussion of indirect business taxes and, in particular, the rather superficial view taken by some of tax incidence (for example, the flat assertion by Jaszi on p. 207 that indirect business taxes leave relative factor incomes unchanged, presumably in contrast to direct taxes). Miss Young's paper seems to be an extremely painstaking piece of statistical work. The last paper, by Kravis, tries to grapple with the old problems of defining economic activity. He has some interesting points to make but, whilst not being as nihilistic as Viner seems to be in his comments, one does feel that somehow the discussion has not been advanced much further. One also gets a slight feeling, which may be quite unjustified, that the author is much happier in his handling and knowledge of ' Western ' than of underdeveloped countries.

A. R. PREST.

Dogmengeschichte des wohlstandspolitischen Interventionismus. By P. KELLER. Verlag P. G. Keller, Winterthur. 1955. x + 367 pp.

In this volume Dr. Keller presents an excellent survey of interventionist theories of economic policy, ranging from the *Polizeiwissenschaft* of the eighteenth century to modern welfare economics. Although there is a good deal of detailed discussion of individual authors, well illustrated with numerous quotations (in four languages), the exposition is always balanced and the main lines of development are well brought out. This clarity of outline is achieved by imposing certain restrictions on the scope of the work. Only policies purporting to increase welfare are considered, and much mercantilism aimed at augmenting national power therefore omitted. Both socialism and extreme *laissez-faire* are excluded, while ' interventionist ' doctrines are only considered where they are general in scope and proceed from a consistent ' formula ' for intervention. Problems of international trade are ignored, as are interventions for macro-economic purposes.

The search for formulæ for intervention is the book's *Leitmotif.* It provides an admirable tool for assessing the merits of the various authors, and leads to a number of controversial suggestions. It serves to distinguish achievement in the field of economic analysis from contributions to thought about policy, and to sort out authors with a consistent doctrine from those with a mere love of authoritarian meddling—*Vielreglerei.* By this test Bentham emerges as the hero of the story, a view which is unlikely to surprise English readers as much as those whom Dr. Keller is primarily addressing. The same is true of his demonstration that the English classical school—in particular, Malthus, McCulloch, Cannan, Sidgwick (whom he rates very highly) and J. S. Mill—were not blind devotees of so-called *laissez-faire*, but on the contrary developed interventionist doctrines. In his survey of the German literature, Dr. Keller does a useful job by sorting out the consistent doctrine from the propaganda of state-worship in the works of Adolf Wagner, and argues that despite his deficiencies Wagner stands above other German interventionist writers. In the final part of the book, dealing with welfare economics, Dr. Keller awards many prizes to Professor Pigou for his originality, systematic treatment and interest in practical questions. After a brief survey of recent developments in welfare economics—already somewhat overtaken by events—Dr. Keller expresses the hope that more attention will be paid to the general theory of economic policy, and the belief that fruitful work would result from a combination of the English analytical and German systematic traditions.

It is a great merit of this book that Dr. Keller combines scholarship with readability; he writes excellent literary German, avoiding the complexities of the more common scholastic style, and obviously has a sense of fun.

LUCIEN FOLDES.

Fluctuations, Growth, and Forecasting. By SHERMAN J. MAISEL. John Wiley & Sons, Inc. 1957. ix + 552 pp. $7.50.

Considering the recent popularity of the do-it-yourself manual it is not surprising that another " forecasting for profit " book, one founded on orthodox economics, should make its appearance at this time. According to the author, an associate professor of the School of Business Administration at the University of California, this book will enable readers " to understand the causes of change and to use this knowledge to choose policies that are most promising in an unstable world ". Despite its 550 pages, the conscientious reader who puts this volume down with the conviction that he is now fit and ready to choose the " most promising " policies must be unduly sanguine. He will have learned nothing much about statistics, nothing at all about econometrics—though he will have been referred to articles in *Econometrica*—

and the fruits of econometric research will have been demonstrated for his benefit by a demand curve for canned cling peaches.

Part I discusses the forces at work in the U.S. economy, and the concepts used in appraising them. What strikes one forcibly at this stage is a sort of obtrusive salesmanship in the presentation: one is subjected to the pressure of short urgent sentences and hard-hitting points clinched by anecdote and illustration. Part II is largely a cursory account of the kinds of data required for decision making, such as national income and expenditure tables, index numbers, their use and interpretation. Part III introduces the reader to the determinants of consumption, investment and government expenditure, followed by some chapters on " case studies " of spending patterns, and ending with a chapter on forecasting prices, output, and employment. Part IV starts with a brief classification of cycle theories, follows with two chapters on the economic history of the U.S., from 1913 to the present, and ends with a chapter on the procedures used in forecasting. Finally, Part V contains a general discussion of government policies and some further insights into the consequences of business decisions.

The emphasis, in forecasting, is on treating separately each of the chief factors at work—a sort of casual extrapolation of each of the relevant series, bearing in mind this, that, and the other—rather than on the construction of formal econometric models (though the reader is invited to avail himself of appropriate econometric equations if obtainable). Despite all that is said on this subject one remains doubtful of the possibility of imparting this rather informal technique through the medium of a textbook. In the case studies we are told of the divers considerations which are weighed judiciously in the mind of the forecaster prior to his alighting upon a certain assumption. This, no doubt, is what happens, and the figure assumed is, as the author points out, reasonable enough in the circumstances. But the figure chosen might have been significantly different without shedding its plausibility to the reader. Judgment, which can only come with experience, is the essence of such a method. Without it the technique is but a toy.

As a textbook on economics for beginners, which is another use the author sees, its value is uncertain. Though it abounds with facts, figures, illustrations, " well known " relationships, general beliefs and differences of opinion among economists, it is not likely to give a new student an adequate impression of the growing rigour of modern analysis. Perhaps a certain frothiness in the theoretical treatment is inevitable in a text whose main burden is to demonstrate continually how everything in the economy does depend on everything else, but in part it is the result of the absence of a clear distinction between theoretical postulates and business practices. The accent, needless to say, is on the latter and, for students in American Schools of Business, this may be considered as one of the virtues of the book. It is less of a virtue for beginners in economics where, from the start, the emphasis should be on investigation and testing of economic hypotheses—not on the enumeration of general business practices.

If he is not put off by the rather oppressive style, a student of economics may, none the less, pick up quite a lot of useful factual information plus some elementary notions of the quantity theory, the multiplier, the acceleration principle, and the like. But for a serious student, such a book should be strictly supplementary reading.

E. J. MISHAN.

Traité de politique fiscale. By Maurice Lauré. Presses Universitaires de France, Paris. 1956. 425 pp. Fr. 1,400.

Compared with the most recent theoretical contributions to the subject, this book on tax policy appears rather dated. It contains a discussion of such standard problems as incidence, discrimination, income distribution and taxable capacity, but only in relation to simple notions of Paretian general competitive equilibrium. The complications arising from so-called second best situations and the existence of market imperfections are not taken into account, nor is proper attention paid to the difficulty of defining social welfare and deciding between alternative tax systems on that basis.

It is, however, possibly a trifle unfair to judge the book on this basis because, while it throws no light on matters of theory, it does contain much of interest on more practical matters. M. Lauré spent the ten years immediately after the war in the service of the French government, and was concerned with the actual administration of taxes and central finance. With this background he is able to comment most informedly on the day-to-day problems of fiscal policy in his country.

He deals, for example, with the vexed question of fraud in the French tax system, arguing that its extent is usually exaggerated. He points out that the propensity not to pay taxes varies between industry, agriculture and commerce, between large concerns and small concerns, and between self-employed people and people working for others. A comparison of the situation in France with that in the U.S.A. leads him to the conclusion that there is less fraud in the latter country, not so much because people there are more honest, but because the sectors of the economy in which there is a higher propensity to avoid taxation are relatively less important. He does, of course, agree that in addition some weight should be given to national mentality.

Another topic which he treats most successfully is the effect of the French tax system on her external position. In particular, France's tax system handicaps her ability to expand the level of invisible exports. He suggests, for example, that compared with other European countries French restaurant and hotel prices are about 10 per cent. too high, and compared with Switzerland 15 per cent. too high.

In a similar fashion he deals with the questions of taxable capacity, equal pay for women, capital gains taxes, and the potential growth of the economy. His general point of view is that the French tax system is inefficient because it is too *ad hoc*, too varied and irrelevant to the major problems of the present day. A rationalisation of the system could have the great advantage of removing many of France's economic problems while interfering less with the free workings of markets and reducing costs of administration.

M. H. Peston.

Cost Data for the Management of Railroad Passenger Service. Dwight R. Ladd. Harvard Business School, Boston. Bailey Bros & Swinfen Ltd. 1957. xvi + 345 pp. 36s.

This book examines the techniques used by various American railroads for the analysis of the cost of their passenger services. The author is mainly interested in the preparation and use of cost data by management in decisions about the withdrawal and extension of services. The federal regulatory authority—the Interstate Commerce Commission—has evolved a uniform system of accounts and all railroads must present their financial records in this standard form. The author shows that these accounts are of little value for managerial decision-making. The I.C.C. accounting rules submerge individual details of cost in general accounting categories which the cost investigator requires to keep separate, and in any case the rules leave quite a wide margin for individual interpretations.

One of the main conclusions is that there must be separate accounts for management purposes quite distinct from those developed for the I.C.C. The author believes that too many generalisations are made about costs. Costs should be ascertained for specific functions; then any service can be broken down into its constituent elements and each function can be assigned a cost to provide a realistic estimate of specific costs.

The most interesting chapters are those in which actual cases of decisions about extension or withdrawal of services and the concomitant cost studies are set out. The author shows that often the use of average costs of the whole railroad to estimate the change in costs due to a withdrawal of a train was most misleading and that comparatively small additional research would have paid handsome dividends.

A disappointing aspect of this study is the cursory way in which the author relates the concepts of cost and the techniques of costing used by railway research and costings divisions to the cost concepts used in the theory of the firm. J. M. Clark's classic warning that incremental costs " do not correspond with any accounting category " is repeated, but in costing practice it has to be ignored. This uncomfortable inconsistency remains with the reader throughout the whole of the book. The important question is whether the " out-of-pocket " or " direct cost " is or is not an approximation to short-run or long-run marginal cost, and in particular whether there is a bias introduced by these techniques. No answer will be found here.

Although rather repetitive Professor Ladd writes in an engaging style, interspersing his narrative with amusing anecdote. All railway administrators will find much that is of value in this book. But, apart from those who are especially interested in rail costs, economists will find little to further their knowledge.

A. A. WALTERS.

SHORTER NOTICES

The Poverty of Historicism. By KARL R. POPPER. Routledge & Kegan Paul Ltd. 1957. xiv+166 pp. 16s.

In 1944–45, Professor Popper in a series of articles brilliantly exposed the fallacies involved in predictions about the course human history must take. These articles deservedly won international fame, and have re-appeared in other countries, but they have not hitherto been available in book form in this country. Although the narrower range of *Economica* to-day precludes any extended notice, we are glad to draw attention to the new publication, as a book, of these important articles which originally appeared in this journal.

Robert Oliver, Merchant of Baltimore, 1783-1819. By STUART WEEMS BRUCHEY. The Johns Hopkins Press, Baltimore. 1956. 411 pp. $5.

Mr. Bruchey gives us a full and workmanlike study of an early U.S. firm. Oliver was an Irish immigrant to Baltimore, who grew rich with surprising speed. His records tell us how he did it—but not what kind of man he was. The pattern of his trade is reassuringly like that shown in other studies of contemporary firms: he was a general merchant, willing to try almost any kind of venture with any part of the world, skilled in barter and the use of bills, and quick to pull in his horns when times grew bad. His accounts were remarkably complete, and show that ' annual profit ' was merely the total gains on such of the many consignment and voyage accounts as could be closed off during the year; and these ventures took so long to wind up that Oliver needed ten years to pay out a retiring partner. The firm's most dramatic feat occurred in 1806–7 when, under Ouvrard's scheme for financing Napoleon with Mexican treasure, Oliver schooners brought fabulous sums of silver out of Vera Cruz.

The Gold Coast Cocoa Farmer. By POLLY HILL. Oxford University Press. 1956. viii + 141 pp. 16s.

Apart from a survey in one village (Akokoaso) in 1932–35 by W. H. Beckett little has been known about the cocoa farming industry of Ghana and even an official report has been misleading. This book therefore helps to fill a marked gap in knowledge and does it very well indeed. It is primarily based on exploratory surveys in ten arbitarily chosen areas recording the employment of workers (farmers, though not often absentees, rarely work their own farms); the establishment of new farms (which take seven years before they bear significantly); and some rather meagre information on the pledging of farms. It concludes with a discussion of the amount of the farmer's income and of his expenditure. A prosperous farmer has responsibilities to a large family circle and funerals are very expensive; he likes a house and a lorry but is not interested in a private car, bicycle or domestic equipment for his house. This is a well worth-while book.

Estimates of Israel's International Transactions, 1952-1954. By NADAV HALEVI. Falk Project for Economic Research in Israel, Jerusalem. 1956. 139 pp. $2.

Estimates are made of the balance of payments of Israel for the years 1952 to 1954 with a precision and with detailed notes intended to make them adequate for use as benchmark data for future estimates. But the work is also in part a study of concepts and methods, and there are warnings about using the figures wrongly and the need for the economist to look elsewhere to understand and explain trends. This is rather a specialist book; previous official estimates give much the same picture of a country very dependent upon unilateral transfers.

The Distribution of Wealth. By John Bates Clark. Kelley & Millman Inc., New York. 1956. xxviii + 445 pp. $8.50.

This is a reprint of J. B. Clark's great book, probably the most ambitious ever attempted on the marginal productivity theory and on static micro-economic marginal analysis. In spite of the great intellectual virtues of the book and the great contribution it made in its day, it seems, at the moment, almost impossible to foresee how the main interest of economists will ever centre again around this kind of work to anything like the extent it did when he wrote it.

Schumpeter's Theory of Capitalist Development. By Mohd. Shabbir Khan. Muslim University, Aligarh, India. 1957. viii + 175 pp.

This essay on Schumpeter's theory of economic development is distinguished by wide reading and lively comment. Schumpeter's relations to the Austrian School, Walras and Marx are traced, and the bearing of his theory is examined with regard to Harrod-Domar's growth formula and Hicks's analysis of the trade cycle.

Productivity, Prices and Wages. By Jean Fourastié. O.E.E.C., Paris. 1957. 113 pp. 10s.

Though not explicitly stated, this book is an argument for the use of the labour theory of value, at least in the long run, whenever products are being currently made and bought. The real price of a commodity can be usefully expressed in terms of hours of work, directly and indirectly, needed for its production, because pure rent and interest are often a small part of aggregate cost. In practice the economist finds it difficult to obtain good measures of the number of hours' work involved in production because of labour involved indirectly, including the labour in the capital equipment. Thus it is often better to find the number of man-hours by dividing price per unit by average earnings per hour. The difficulties and advantages of this method are discussed together with problems of weighting when trying to generalise from the single commodity to the complex of commodities in overall production. The method and the illustrations (usually where differences are extreme) are well worth examining and the exposition is simple.

Statistics in Theory and Practice. By L. R. Connor and A. J. H. Morrell. Fourth edition. Pitman & Sons Ltd. 1957. xii + 249 pp. 30s.

The fourth edition of this very good elementary text-book has been thoroughly revised by Mr. Morrell. The revision was long overdue: the ' Practice ' part of the book, which consists of descriptions of published statistical data, has been brought up to date. Now that this part of the book has been completely rewritten, it is again a book to recommend to students. The revision of the ' Theory ' chapters has led to some compression. It seems unfortunate that sections on interpolation (apart from the calculation of the median and quartiles), Pareto and Lorenz curves, contingency and a slight reference to the analysis of variance have been omitted, for these are likely to tempt a student further. Sections on control charts and acceptance sampling have been added.

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